

CHEMICAL HERITAGE FOUNDATION

FREDERICK T. WALL

Transcript of an Interview
Conducted by

James J. Bohning

at

La Jolla, California

on

21 June 1991

(With Subsequent Additions and Corrections)

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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FREDERICK T. WALL

1912 Born in Chisolm, Minnesota, on 14 December

Education

1933 B.S., chemistry, University of Minnesota
1937 Ph.D., chemistry, University of Minnesota

Professional Experience

University of Illinois
1937-1939 Instructor
1939-1941 Associate
1941-1943 Assistant Professor
1943-1946 Associate Professor
1946-1964 Professor
1950-1963 Chairman, University Research Board
1955-1963 Dean of Graduate College and Research Professor

University of California, Santa Barbara
1964-1966 Professor and Chairman, Department of Chemistry
1965-1966 Vice Chancellor for Research

University of California, San Diego
1966-1969 Professor of Chemistry, Vice Chancellor for Graduate Studies and Research
1981-1991 Adjunct Professor of Chemistry

American Chemical Society
1969-1972 Executive Director

Rice University
1972-1978 Professor of Chemistry

San Diego State University
1979-1981 Lecturer in Chemistry

Honors

1945 Award in Pure Chemistry, American Chemical Society
1959 Outstanding Achievement Award, University of Minnesota
Member, National Academy of Sciences
Member, American Academy of Arts and Sciences

ABSTRACT

Frederick Wall begins the interview with a discussion of his family background and childhood in Minnesota. During high school, Wall developed an interest in chemistry and mathematics, and planned to become a chemical engineer. He attended the University of Minnesota, studying both chemistry and chemical engineering. One of his professors there, George Glockler, influenced both his decision to focus on physical chemistry and to pursue graduate work. After graduating with a B.S. in chemistry in 1933, Wall was awarded an assistantship at Caltech, which he accepted. Due to financial difficulties exacerbated by the Depression, he only spent a year at Caltech. While he was there, however, he was greatly influenced by Linus Pauling. Wall moved back to the University of Minnesota, and continued his graduate work under Glockler. He earned his Ph.D. in chemistry in 1935, and soon thereafter accepted a teaching position at the University of Illinois. He began working on infrared spectroscopy, and did some theoretical work on covalent and ionic character. Gradually, he became interested in polymers, and when World War II broke, he volunteered to work on the rubber problem. Carl Marvel and Roger Adams then helped Wall to get a consulting job with DuPont, which he continued for many years. In 1955, he became Dean of the graduate college at Illinois. In 1963, Wall decided to leave Illinois and moved to the University of California at Santa Barbara, where he became Chairman of the chemistry department and Vice Chancellor for Research at Santa Barbara and later Vice Chancellor for Research at San Diego. In 1969, he became executive director of the American Chemical Society (ACS), but soon rejoined academia, becoming professor of chemistry at Rice University. At Rice, Wall resumed his theoretical polymer research, particularly polymer configuration on lattices. Seven years later, he moved back to California, taking a lecturing position at San Diego State University, and in 1981 becoming an adjunct professor at the University of California at San Diego. The interview concludes with a discussion of his time at the ACS and his colleagues in California.

INTERVIEWER

James J. Bohning is currently Visiting Research Scientist at Lehigh University. He has served as Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has written for the American Chemical Society News Service, and he has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995.

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INTERVIEWEE: Frederick T. Wall
INTERVIEWER: James J. Bohning
LOCATION: La Jolla, California
DATE: 21 June 1991

BOHNING: Dr. Wall, I know you were born on December 14, 1912, in Chisholm, Minnesota.

WALL: That's correct.

BOHNING: Could you tell me something about your parents and your family background?

WALL: My parents were both born in Finland and emigrated to the United States around the turn of the century. My father came, I think, about a year before my mother to find a place to work and live. Many of the Finnish immigrants at that time went to the northern tier of states: northern Minnesota, northern Michigan, some went to New England. Northern Minnesota resembles Finland in its lakes and woods. They saw that not as a place given to luxurious agriculture, but as a place where they would have to work to make a living. It was what they were used to: actually, my father was not a farmer, he was a baker. His father had been a miller, working a little mill—I don't know whether he owned it or not. My father started and maintained a bakery in Chisholm.

My father came to this country and settled in the village of Chisholm, which had a sizable Finnish community. In fact, a number of the towns on the old Mesaba range have a great many Finns. They went there to get started, not because the jobs available were what they liked, but because it would give them a way to get going. For example, there were iron mining jobs opening in the excellent Minnesota iron mines. They had wonderful ore—now largely depleted, you understand, but at that time the supply seemed inexhaustible. There were lots of jobs. So a great many immigrants went there, including Finns who went not because they liked mining—that's not their inclination—but because it was a way to get started. Their aspirations generally were to get a tract of land for farming. It didn't necessarily work out that way because their children, in the second and third generations, became assimilated and were encouraged to go to college and do other things, as well.

I was the last of six children. My two oldest siblings, a sister and a brother, were born in Finland, and the others were born in this country. The eldest of the family, a sister who just died last December, was well over ninety when she died. I was the youngest of the family, and I'm the only survivor at this time. So that, very briefly, is some of the background.

A lot of people ask, “Where’d you get your name?” If they know anything about names, they know Wall is not a Finnish name, and indeed it is not. They ask, “What was it changed from?” Well, it wasn’t changed in the USA, nor did my father ever change it. Wall was my father’s name, and only a few years ago did I learn its origins.

I have a distant cousin, who’s a professor of Latin at the University of Helsinki, whose father had corresponded with mine. His son was a graduate student at Caltech, and he hoped that we would meet him. Then I learned from his son how we got our name. It turns out that one of our forbearers, I’ve forgotten how many generations back, had been a drill sergeant in the Finnish Army. His commander, who was probably a Swede at that time—because Finland for a long time was a part of Sweden—said he didn’t like those long Finnish names, polysyllabic names with many vowels. Every non-commissioned officer had to have a name that had some military significance, so this forbearer of mine suggested that he adopt the name Vallitus, meaning in Finnish a parapet, which is a kind of a wall, but that’s coincidental. But the commander said, “Vallitus? Well, yeah, but that’s still too long, so why don’t you make that Wall?” It was then to be spelled with a W, pronounced like a V, and indeed the name became Wall.

BOHNING: That’s interesting.

WALL: The Finns don’t like words that end in l. In general, they don’t like words that end in consonants, except s and n. For example, common Finnish names end with nen; if you see that at the end, that’s probably a Finnish name, like Laitinen. Also, s can end a word, but l, almost never. My parents’ Finnish friends would call us the Valli family. (Valli could indeed be used for Wall.) The letter i was at the end for the sake of Finnish euphony. Anyway, that’s the story of our name. I was glad to learn about that because people so often ask me about it.

Actually, my father was substantially monolingual. He didn’t go through high school or the equivalent. My mother was bilingual, as so many Finns were at that time since both Finnish and Swedish were official languages. Although she spoke Swedish as well as Finnish, her surname was Rauhala, a typical Finnish name.

BOHNING: That’s excellent. Did you grow up in Chisholm?

WALL: Until I was about eight years old. I went through third grade in Chisholm and then we moved to Minneapolis. My father retired. It seemed kind of premature by today’s standards, but he was tired of the business. He didn’t quit activity entirely, but he became involved in real estate. He built new houses and bought old houses and rebuilt them for rental. It was not highly remunerative, but it did keep him busy and it provided some income. Moreover, my oldest sister was going to the University of Minnesota, and my parents thought, “Okay, we’ll live in

Minneapolis, and that'll be a good place for the kids to go to school." My parents, especially my mother, encouraged us to go to school, although we didn't get the kind of counseling you would get from experienced counselors. They didn't have my academic background, but still my father and mother always felt that the kids should learn all they could. So we went to Minneapolis, and I went on to finish grade school and high school, and then went to the University of Minnesota.

BOHNING: Did all of your brothers and sisters go to the University of Minnesota?

WALL: Well, my oldest brother went to Carleton College for a while, but he did not finish. My older sister, the one who just died last year, went to the University of Minnesota, and two of my brothers got degrees there. As a matter of fact, one of my brothers got a Ph.D. from Minnesota, as well. I had another younger sister who died, when she was still relatively young, of a heart condition. I never learned precisely what was wrong. She died when she was eighteen. She did not reach the age to go to college; she hadn't even yet finished high school because she had been bedridden for quite a while.

BOHNING: You had quite a precedent ahead of you.

WALL: As a matter of fact, life was harder for the older ones, and I profited from their experience. I suppose, to a certain degree, the other brothers thought I was being favored because my parents were doing for me things they hadn't done for them. That is certainly true, but it was the result of experience, and readjustment, that made things easier for me.

BOHNING: Did you have any teachers in grade school or high school who had an influence on you?

WALL: I'm sure they all, to varying degrees, had some influence, but if you were to ask whether any had a profound influence, then I would say no. I would say that, looking back, I did not get any scholarly encouragement through counseling in high school. I would be asked, "Fred, do you want to go to college?" "Yes." "Well, that's fine." "Yes." "What are you interested in?" Well, I thought I was interested in chemical engineering. "Well, that's mighty nice." Words to that effect. I never received any pep talk about lofty goals.

Actually, I don't know why I was never really encouraged in high school since I was a good student. I was not at the top of the class in high school, although I was on the honor role. (I was probably at the top in elementary school.) We lived not far from the university, so my high school, which covered a large area, included many students who came from the university faculty community. The university community kids had it all over us in terms of sophistication and in the realization that there are things that can be done, things that hadn't been pointed out

to me. This was not the fault of my parents. They just didn't know; they encouraged me to go on, but they certainly did not say, "Why don't you plan to get a Ph.D.?" or something like that. I'm not saying they were unintelligent; that just wasn't a part of their life. But my high school did not fill the gap.

My older sister had married a lawyer who went into academic work, ultimately ending up at the University of North Carolina. They encouraged me and provided an insight into the goals that might be achieved. Then I began to feel that I did not have to settle for something ordinary; I wanted to see if I could do more. I went to the University of Minnesota, ostensibly to study chemical engineering.

BOHNING: How did you develop that interest?

WALL: I was interested in the chemistry.

BOHNING: Was that through your high school years?

WALL: Through high school, yes. I was interested in chemistry and in mathematics. I always had a knack for mathematics, and even as a child, I loved geometry. I liked deductive systems, but if somebody said, "Why don't you become a mathematician?" I would ask, "What does a mathematician do for a living?" Back in those days, unless I had been told that I might become a professor some day, the thought would never have occurred to me. I had to get a job. I knew what it meant to make a living, so I said, "All right, chemical engineering. I like chemistry more, but engineers get jobs." Chemical engineering seemed to offer a practical way of doing something that related to what I liked. Do you understand?

BOHNING: Sure.

WALL: Happily, at the University of Minnesota, the curricula for chemistry and chemical engineering were identical in the first year. Then in the second year, we were required to take one course in chemical engineering, just as a sort of introduction. The third year, things began to split apart, and chemistry majors went more for pure science than the chemical engineers. It was at the end of the first year that I said, "I don't have to stay in this chemical engineering curriculum. It isn't going to cost me any time, money, or anything else. I think maybe I want to go into chemistry." In the second year, I continued the program common to both curricula, plus the brief course in chemical engineering. But I was already convinced not to major in chemical engineering. So in my junior year, I switched my curriculum from chemical engineering to chemistry and lost no time. This was important, because I could not fool around and be a young

dilettante, shifting about at a young age. During my junior year I had decided that someday I was going to get a Ph.D.

BOHNING: How did you reach that conclusion? What made you so certain?

WALL: Well, I had reached that conclusion when I found out that it was possible to have a livelihood even when you're doing things you like to do. [laughter] That's really what it came down to. In other words, I had abandoned the notion that you had to be a practically oriented individual dealing only with mundane matters for manufacturing things. I realized that there was room for a person who wanted to learn and who could contribute to scholarly activities. I had gained confidence simply from having certain associations and some nudging from my older sister and her husband. I put more emphasis on chemistry. Incidentally, I also had a real strong bent towards physics and mathematics, but I never thought of making still another shift. You see, all that time I was subject to the realization that I had to make a go of life.

BOHNING: Well, the Depression was on too.

WALL: Oh, yes. I'll tell you more about that presently; the Depression was starting. In my junior year I had the pleasure of taking a course from a fellow by the name of George Glockler, who was very much interested in research in physical chemistry. I did very well in organic chemistry when it came to understanding the subject, but I was not good in the organic laboratory. I was never a good experimentalist, and in organic chemistry you had to have the knack of a skillful cook, for the instrumentation then available wasn't all that it is now. Although the chief of the organic chemistry division at Minnesota thought I should become an organic chemist, Professor Glockler thought it was fine that I wanted to become a physical chemist. At any rate, that's what I decided to go into.

Well, I talked to Glockler about graduate work, and he was pleased, of course, to discuss the alternatives with me. He suggested that I apply to various schools, and I did indeed apply to several places, including Caltech. At any rate, I got an offer of an assistantship at Caltech. This was in 1933. The Depression was really hard. Unemployment was very high indeed, and in my graduating class practically nobody got a job. Of all the chemistry majors, I think there was one student who got a "regular" job. That was really depressing; I don't think people now have any concept of the depth of that Depression. However, I was offered this assistantship at Caltech and they would provide room and board and one hundred dollars—for the year, not a month. [laughter] I have to point that out because you might ask, "A hundred dollars a month? Well, maybe you could then scrape by." One hundred dollars for the year was my stipend, and I accepted.

I went there with a letter of introduction to Linus Pauling. I talked with him and got to work in his group. Actually, "big science" had not yet developed as we know it today. Now you

have teams with a senior professor managing a whole bunch of post-docs who then tell the graduate students what to do; something like that had only started at Caltech. My immediate supervisor was a post-doc named Lawrence Brockway, who got his degree from Linus. Linus Pauling was the professor in charge, but Brockway was telling me and a couple of others what we were supposed to be doing. That was my first exposure to what was the beginning of what became a pattern in organized university research, but done without recourse to a National Science Foundation. There were just some organizations, like the Guggenheim [Foundation] and Rockefeller [Foundation] and perhaps others, that gave support. I've forgotten who supported Pauling, but he did get outside support.

Well, I couldn't make a go of it financially. I was reluctant to borrow more money from my parents, who had supported me, but not to the point of making things easy. The Depression was rough, and I was fully aware of the financial plight of my parents at the time. In retrospect, I should have borrowed more money, somehow or other, and continued at Caltech. Nevertheless, I returned and I got a job at the University of Minnesota. They knew me and they gave me an assistantship there and I could live at home, so it worked out all right.

I learned a lot at Caltech. I got my first exposure to quantum mechanics from Pauling, who, of course, was a great teacher. I learned some more about thermodynamics from a fellow named [Roscoe] Dickinson and took a course in special relativity from Richard Tolman. Tolman was perhaps the greatest teacher I'd had up to that time. He was phenomenal when it came to explaining things in a clear manner. He could put things in a simple form to show the essence of what was involved. In the case of special relativity, the mathematics is not hard, but the concepts are another matter. Tolman had the capacity to get you to see the meaning of things. This course was in the spring quarter and I was already committed to return to Minnesota, but I began to have second thoughts. I thought, "With a man like Tolman, why would I not make every effort to stay on and continue and hear the next three quarters on general relativity?" However, that was water over the dam.

BOHNING: You did get a paper with Brockway, though.

WALL: Yes, I did. This is what I was going to explain. I could have obtained a master's degree if I had written some more. I really didn't want a master's degree; I wanted to work for a doctorate, which I ultimately received. But I would indicate in my curriculum vitae and so on that I had been a student at Caltech. Then some curious things happened. First of all, some people said I had a master's degree from Caltech, which I didn't. Then I had to correct them because I wanted the statements to be honest. Then some people said, "You know, Fred Wall looks like a pretty good student, but why couldn't he make the grade at Caltech? Is he any good?" Then I'd try to explain it away. So then I stopped saying I ever was at Caltech. I'd gotten to the point where—as much as that experience was a good one for me—I didn't like to have to explain to people why I left. If I gave them an honest answer, they'd say, "Well, it's probably a rationalization." In this connection, I can give you a little interesting story that occurred a number of years later.

My relations with Pauling were always good and I had occasion to introduce him at the dedication of the chemistry building at Santa Barbara. I went up there and I introduced him. Pauling got up and said, "Well, thank you, Fred, for your introduction. That was nice." And he said, "I've known Fred Wall for a number of years and, you know, he was one of our failures." [laughter] I could have sunk right to the floor! But then Linus said, "Oh, I don't mean to say he failed; I mean we failed. We failed to provide the conditions that would have kept him at Caltech." But, for that instant, I . . .

BOHNING: I can imagine!

WALL: Well, when you reflect on it, Linus wouldn't have hurt me. It was a joke, but for an instant, it hit me.

BOHNING: Absolutely, yes.

[END OF TAPE, SIDE 1]

WALL: I'll have more to say about Linus Pauling later.

BOHNING: During your year at Caltech, who were some of your peers in that Pauling group?

WALL: Well, there was a fellow by the name of Bill Medlin, who I think went to Chevron. Then a fellow by the name of John Youngs Beech who went on to Princeton for a while and then ended up with Chevron, or Standard of California as it was called then, in the Bay area. Then there was a fellow by the name of Fred Stitt who got a post-doctoral appointment at Harvard and then went on to Indiana. I haven't heard much lately about any of these fellows. There was a post-doc by the name of David Harker who did some very good work since then. The name may ring a bell with you. The year I was there, E. Bright Wilson was there. He was working with Pauling in a post-doctoral capacity.

BOHNING: Oh, yes.

WALL: Wilson would sometimes fill in for Pauling's lectures.

BOHNING: Pauling and Wilson took shape around that time.

WALL: Exactly. Anyway, it was a very important year for me, notwithstanding the fact that I hadn't followed through. It was a very important year for me because it gave me a measure of scientific sophistication that I would not have achieved otherwise. It was a way of learning what it means to be a scientist in this world. There were some interesting associations resulting from the room and board arrangement. Assistants could live either in the loggia lodging of the Athenaeum or in an old wooden dormitory building. I elected the old dormitory building, where I had a room, because I just felt more comfortable by myself. I was somewhat withdrawn, as I have been most of my life; anyway, I preferred privacy. We had our meals at the Athenaeum, and I remember a joke about it. There'd be articles in the Pasadena paper about the fine Athenaeum, a very nice place, and how a lot of people wished they could gain membership. Whereupon I made the remark to somebody, "Suppose you didn't want to join, could we be around here yet?" [laughter]

But it was an enlightening experience because you could really get a feel for things. Even though there was a depression, there were distinguished foreign visitors who came through, and you would hear them talk, or perhaps see them in the Athenaeum dining room. The reason why the assistants were obliged to eat in the Athenaeum is that Caltech had just completed the building and they didn't have enough money to run the thing. It was a matter of handling some of the overhead by paying off TAs in part through Athenaeum membership. That was probably the real reason, which is understandable.

Anyway, I returned to Minnesota and continued my graduate work. I did my research with Professor George Glockler. He was the one who advised me as an undergraduate and was the one that recommended me to Caltech. The national economy started picking up along about 1936-1937, and, happily, I received a number of job offers, but there remained the question of what kind of job I wanted. I wanted an academic job at a place where there was opportunity for research. I heard about an opening at the University of Illinois, and, to make a long story short, I did get a job there as an instructor. This was in the days before universities hired people as assistant professors to start with. I started one step above the TA; that was in an instructorship. Illinois had, of course, a distinguished and highly respected chemistry department.

BOHNING: Before we talk about Illinois, could we just review a little bit more of your graduate work at Minnesota?

WALL: Oh, sure.

BOHNING: What kind of a person was Glockler to work for?

WALL: He was a very good person to work for, and he would be really helpful. He wouldn't do your work for you, but if you really had a problem he would be happy to discuss it with you, and he was exemplary in terms of doing things. He was an exceptionally good experimentalist who could construct equipment and make things work much better than I could. He was moderately good at theory. I think my understanding of theory was beyond his by the time I was through graduate school. I'm not trying to run him down. I knew something about quantum mechanics thanks to Pauling, and this is back in the days when quantum mechanics was just emerging as useful for chemistry. You certainly didn't teach it to freshmen. Well, I learned some of that from Pauling, and that was a big step forward. Then I had that positively exemplary course in special relativity from Tolman, which made me feel good.

BOHNING: Did you have any more formal exposure to quantum mechanics at Minnesota?

WALL: I did not. I'm trying to remember, but I didn't take any further courses in quantum mechanics at Minnesota.

BOHNING: What about mathematics?

WALL: Mathematics? I didn't take any formal courses, though I sat in on a course on group theory, and that was helpful. After receiving my bachelor's degree and in the summer before I went to Caltech, I actually went to summer school at Minnesota for two reasons. First, I needed to study French. I had studied German, which was required of all chemistry majors. That was the foreign language you had to have, but I didn't have any French and I knew I'd need that for my Ph.D. program.

So I took a summer session course in French, and at the same time I took a course in special topics in mathematics so that I might learn some more about differential equations. The summer session was a good one. I got a start in French, and with some further study on my part, I reached the point where I was able to pass an examination in reading French.

The mathematics turned out beautifully. The instructor handed me a book and said, "Why don't you take a look at some of the problems here? Work them out and hand them in." Well, I went through the whole book, worked all the problems and handed them in: he was amazed. Since I had done every single problem in that book, he turned me over to one of the other mathematicians, who started telling me about orthogonal functions, generating functions, and things like that. I lapped it up, and it was real fun. That, incidentally, helped me understand quantum mechanics better, because when I got into Pauling's quantum mechanics course, I didn't have to ask what was meant by orthogonality or by normalization. When I was through, I got As from both of my math instructors, and they said, "You've done more work than what you registered for, so you may get more credit than that." I said, "That would be very nice." Not that I needed the credit. But there was a rule that said you can't have more than so many credits

in a summer session, so I petitioned to get my French credit voided. I said, "I'm not looking for credit in French. I wanted to learn to read French." So then they upped my math credit instead. [laughter] I did not need that credit either, as it turned out, but at that time I thought it might be useful for Caltech. So, that summer session was very useful, and it contributed something to my picking up quantum mechanics.

BOHNING: That's fascinating. You had four papers with Glockler (1).

WALL: Yes.

BOHNING: Which is really quite a bit out of one thesis.

WALL: Maybe. One of them we had talked a bit about, but I wrote up most of it and showed it to him; he thought it was great, we discussed it, and had it published. That was the first time I really wrote a paper. I worked on all of them, but there was one where I was almost the sole author. In fact, he said, "Your name ought to be first." I said, "Oh, I don't think so, Professor Glockler. After all, I'm learning, and without your advice and counsel, I wouldn't have done this." So, that was it.

BOHNING: He was more accessible than Pauling was. He didn't have a hierarchical arrangement.

WALL: He didn't have a hierarchical arrangement, and he was certainly very helpful. There were several good teachers at Minnesota. The fellow in charge of organic chemistry, Lee Smith, was a superb teacher, and he was disappointed that I didn't want to become an organic chemist. Another good teacher, who appeared to be lackluster but was very rational and logical, was Professor [Frank] MacDougal, who taught thermodynamics and did a very good job, indeed. Lee Smith did a lot of research, too, as did Glockler, but MacDougal didn't do much research. He was the work-horse teacher in physical chemistry.

BOHNING: How about some of your peers in the Glockler group?

WALL: Well, let's see, there was Malcolm Renfrew.

BOHNING: Oh, yes. I talked to him (2).

WALL: Oh, you did? I believe he went to DuPont and then on to the University of Idaho. Malcolm Renfrew shared an office with me as a graduate student.

BOHNING: Okay.

WALL: Let's see, I'm trying to think of the others. Well, there's a fellow by the name of Art Wishart whom I knew; I don't know where he ended up. I think he ended up in Florida. Then there's a man I knew very well but lost complete track of, his name is Francis Martin. He was very intelligent, and he was the one I was always competing with as an undergraduate to try to be, if possible, the top of the class in chemistry. He was very smart, very capable in mathematics, and I had great respect for him; but unhappily, he was inarticulate. I thought he knew more than he was able to demonstrate or to sell. He could not give a good seminar, and the upshot was that his ability was lost because he just couldn't convey thoughts effectively. A capacity for expression enables some people to look smarter than they are; in his case, he was smarter than he appeared.

There is another man, not a classmate of mine, who ultimately became pretty well known; he was an excellent student named Ed Piret. Piret worked in industry and for the government. His governmental work involved much in France because he spoke French fluently. He got some kind of a doctorate from Lyon [University] as well as his Ph.D. from Minnesota.

I do remember something that may give some insight into my inclinations or disinclinations. During my undergraduate days, the University of Minnesota had a number of fraternities and sororities—not as many as at Illinois, but they were a sizable number, and they had their campus politics. Every once in a while, a dissident group would arise and say, “To heck with these fraternity and sorority people.” Thus, you would have Barbs (meaning non-Greek barbarians) running for student offices. I remember in my junior year, the Barbs wanted me to run for some student council, but I figured I didn't want to get involved in it, although the thought had some appeal. I was a Barb by attitude. There was a professional fraternity for chemists, Alpha Chi Sigma, that made a strong bid for me to join, but I declined. They were very civil about it, but I just didn't feel as though I cared for fraternities. Maybe I didn't know enough about it, but I just wasn't moved in that direction.

Anyway, the Barbs wanted me to run for the student council. I didn't, but we got somebody else to run, and he was elected from our area. It turned out by coincidence that two rival Greek groups were almost tied and the Barb had the swing vote. (All the more reason why it was a good idea I wasn't there.) Well, it led to some nonsense. Our Barb appointed some committee people, and in deference to my support I was even made chairman of the refreshment committee for the senior ball. Of course, I didn't do anything as chairman or even go to the ball. I made a joke that we would get a barrel of coffee and some hamburgers and let it go at that. The whole idea was a sort of a little joke, and I was glad to go along with that. I said, “Sure, I'll

be chairman of the refreshments committee for the senior ball,” as if it made any difference. [laughter] It was a chemical engineer who was our Barb representative.

BOHNING: Before you got the position at Illinois, did you interview any place else?

WALL: Yes. I remember, by coincidence, I was able to combine a number of visits on one trip. Back in those days universities weren't inclined to pay expenses for applicants. I was invited to visit the Mellon Institute, which was then a research institute not yet officially affiliated with the University of Pittsburgh. I went there and talked with some Pittsburgh Plate Glass Company representatives. They were very nice, and they tried to persuade me to accept an offer. I said, "To be perfectly honest, I would prefer an academic position." They said, "You can have a job here, and you can associate with the people over at the University of Pittsburgh. We'll help you do that and maybe we can get some kind of joint appointment." They were very nice, so I didn't turn them down because I wanted to look further. There was also an inquiry about me from Eastman Kodak. But Samuel Lind, the dean of the school chemistry, said, "We don't want Wall to go to industry; he belongs in academics." So I never visited Eastman Kodak. Nevertheless, I made the trip to Pittsburgh, and arranged to come back by way of Ann Arbor, Michigan, where I talked to people in their chemistry department, and then I went down to Urbana, where I talked to the people in Illinois. Then I returned to Minnesota; the largest share of the expenses were paid by Pittsburgh Plate Glass, but the side trips I bore myself.

Do you mind my rambling about little bits of things?

BOHNING: No. Not at all.

WALL: I had a hobby of collecting railroad timetables and I had a formidable collection. Incidentally, I regret now that I didn't keep it, but I was collecting it just for fun. The fact of the matter is that you can collect almost anything and, if you hold them enough years, somebody will say, "This is valuable." I had collections from all kinds of wooden axle pikes around the country, including for the big ones. I had enough of them so that they would be a collector's dream now.

I would go to stations in any city I happened to be in and I would always take a look to see if there was a timetable I didn't have; I could spot them from a distance if they had them on a rack. I also wrote for some.

At any rate, I wanted to ride on the Ann Arbor railroad, which went from Toledo, Ohio, through Ann Arbor and up to the head of Lake Michigan, cutting diagonally across Michigan. It was principally a freight road. Now, the best way to go from Pittsburgh to Ann Arbor was to go to Detroit and take the Michigan Central across to Ann Arbor. Any normal person would do that. Well, I knew that was the best way to go, but I wondered if I would ever be able to ride the

Ann Arbor railroad. So instead I went to Toledo and then took the Ann Arbor from Toledo. They had a mixed freight with a shabby passenger car.

I don't know what the reaction was from the people at Michigan. They asked, "When did you come in?" I said that the train got in at such and such a time. They remarked that they didn't know there was any train at that hour. I said, "Oh, sure. The Ann Arbor line." Perhaps they discredited me for that. It was an idiosyncrasy of mine to just see how many different little out-of-the-way pikes I could ride on. Maybe they thought I was too dumb to be hired by them. Anybody would be stupid to take the Ann Arbor railroad... At any rate, I did that and then took the Michigan Central from Ann Arbor to Chicago and then the Illinois Central down to Urbana.

Anyway, I had a pleasant interview with the people at Michigan. Then I went down to Illinois. I don't remember my communications with Michigan, whether they sent me a letter of regret before I got the job from Illinois, or if it was more or less simultaneous. I did go to Urbana to talk with Worth Huff Rodebush there, who introduced me to Roger Adams. We seemed to hit it off pretty well, and after I had talked with Roger Adams, I talked with Rodebush again. Rodebush was in charge of physical chemistry and told me, "You're going to be hearing from us pretty soon. We have to clear this with the dean." He also said, "Dr. Adams has looked at you, and he can't see anything wrong with you. He said we should go ahead with it." [laughter] Adams was the grand old man of organic chemistry and the head of the department at Illinois.

I didn't hear from Rodebush right away, and I was getting a little antsy. Actually, I should have known that his word was good. It was informal, but I wanted to hear from him, and finally the letter came with the job offer.

[END OF TAPE, SIDE 2]

WALL: They hired [Lawrence] Brockway at Michigan. Now I recall that they said they wanted someone with additional experience.

BOHNING: Oh, really?

WALL: Yes, I'm pretty sure he was the one. After hearing from Illinois, I informed the people at Pittsburgh Plate Glass that I was sorry I couldn't accept their offer, and I thanked them for their consideration. There may have been one or two other companies I dealt with. Oh, yes, I had already accepted the job at Illinois when I got an inquiry from one of the departments at DuPont. I believe I was at a lake in the northern part of Minnesota, where my older sister and her husband had a cottage, when a wire came from what was then called the Ammonia Department. I sent a wire back indicating that I was already committed to the job at the University of Illinois. I thought that was a happy circumstance for me, but I never failed to

recognize that I was lucky, too. There are too many things that can go sour at times; you just can't take them for granted. And the timing was right because of the pickup of business and the surge of hiring. Thus I was able to get the kind of job I wanted at what was regarded as one of the best chemistry departments in the country, albeit the citadel for organic chemistry; I am a physical chemist, and I knew that Illinois did not have one of the best physical chemistry departments in the country, by any means. However, the physical chemistry at Illinois was very good and the opportunities were excellent.

BOHNING: You had mentioned Lind before. Do you have any comments about Lind?

WALL: Yes. Samuel Lind was perhaps the most distinguished person on the faculty at the University of Minnesota. You didn't see much of him because there was a lot of demand for his time. He was one of the early workers in radioactivity. At one time, he worked in Madame [Marie] Curie's laboratory, but I think he obtained his doctorate somewhere in Germany. Anyhow, he went from there to Madame Curie's laboratory in Paris. He worked on the effect of alpha and beta rays on chemical reactions prior to the nuclear bomb work of World War II. He knew a great deal about separation of radioactive elements. Back in those days, people wanted radium, which had to be separated out from uranium ores.

He used radon as his α -ray source. He was very much an authority on the radioactive decay families and on what was then known about chemical reactions induced by radiation; he knew something about the biological effects of radiation. I remember his course on radioactivity, which I took as an undergraduate. He talked about the dangers of radioactivity even back then. I remember a rather poignant story he told about watches with luminous dials painted with some radioactive material to make the numbers glow. I remember his saying, "They have women in factories to paint the numbers, and they would tip their brushes between their lips to get a fine point." He was very concerned about the practices, but there were few government regulations about such matters back then. By today's standards that earlier practice would be absolutely inexcusable.

BOHNING: Yes.

WALL: Then he also told a very interesting story. He said, "You could get by with thorium B on these watches, and thorium B would be cheaper, but such efforts are blocked by establishments set up for separating radium." He added, "Thorium B has a half life of only a few years—radium, nearly two thousand years. The half life of a small watch is much nearer that of thorium B than two thousand years. [laughter] Of course, it would be more rational not to use any radioactive material, but if one is to be used, it would make more sense to use thorium B.

I remember Samuel Lind making these statements; it's curious how one remembers such things; they do have a certain kind of impact.

BOHNING: Yes, absolutely. When you got to Illinois, what were your first teaching assignments?

WALL: Oh, my first assignment—and this was ostensibly what I was hired for—was to teach physical chemistry to pre-medical students. Back in those days, prior to my going there, the pre-medical work required only two years of liberal arts and sciences. Then a student could go to medical school. Well, they changed the pre-med requirement from two years to three years, and one of the things that would be involved in the third pre-med year would be physical chemistry. There would be some other requirements as well, including more biology and so on; but the one I remember is physical chemistry because that's what I was to teach. The pre-med course worked out pretty well, but it got a reputation for being a course that was designed to eliminate poor students. This feedback was not displeasing to me since I didn't mind people saying, "This is a course you have to work hard at." That made me feel good, and if they said I was a good teacher, and some did that, that made me feel good, too.

I had a second teaching job, too, and this had to do with drawing on some of my experience at Caltech. I would talk about the nature of the chemical bond—something I had learned from Pauling, of course, and which I also had studied by myself—and various other things. This was a graduate course; the undergraduate one was for pre-meds.

I taught those courses for a number of years. Meanwhile, I was getting on with my research, although I did enjoy teaching. I should explain that at the end of the first year, I went in to talk with Roger Adams about how my first year had been. I usually didn't have much of a chance to talk with him, so it was kind of a treat. You know how it is: a man up in the clouds, with whom you only get to talk occasionally. I think he may have called me in, as he did to all the other youngsters, and he asked, "How are things?" I said, "Well, fine." "What have you been doing?" Well, we talked about various things, and then I made a statement about teaching. I said, "You know, I enjoy that teaching." He looked at me kind of strangely and said, "You enjoy teaching?" That look was enough to persuade me that I shouldn't have said it. I should have emphasized the research, which I also enjoyed. Only years later did I tell Roger Adams again that I liked teaching because I then had nothing to fear.

Now, let me assure you that I had a great regard for Roger Adams and I appreciated the things he did for me. But that incident shows where he put his money and it contributed to the feeling often cited that universities emphasize research and scholarship and let teaching go to the dogs. He made a statement which sounded that way to me and which had an impact on me. Not that I heeded it. I did not heed it, but I never bragged about teaching. [laughter]

BOHNING: It's amazing.

WALL: It's rather amazing, really, because I would have expected him to say, "Well, it's nice that you enjoy it," but he did give me that kind of, "how come?" look.

BOHNING: What kind of research problems were you starting on?

WALL: First of all, I did some work on infrared spectroscopy, which was done largely in collaboration with Rodebush, who had acquired an infrared grating and spectrometer (3). He was very generous in letting me work with it. I also started in on some theoretical work. I did a couple of things on covalent and ionic character.

BOHNING: Yes, you had a paper, "Ionic Character in Diatomic Molecules" (4).

WALL: That's right, yes. I started doing some theoretical work and then I got into theoretical work on polymers in a very interesting way. There was a distinguished organic chemist at Illinois by the name of Carl Marvel—usually called "Speed"—who had a problem about reactions in polymers that called for a statistical analysis. I doubt that he used statistical analysis, but he said, "You know, this reaction goes so far and then it stops and won't go further even if possibilities remain." There was a statistical explanation. I won't go through all the details. Anyway, he sent one of his students around to talk to me, and the student wanted to know why this unusual situation prevailed.

Then I started to work on it, and I soon found that [Paul] Flory had done something similar, so I studied what Flory had done (5). Then I extended it and did some research of my own (6), and then I got very much interested in polymers and the possibility of using statistical methods in connection with describing polymers (7). This was an important step in relation to my future research, because it led ultimately to a theory of rubber-like elasticity in conjunction with calculations about the sizes and shapes and configurations of rubber-like molecules. All this was an outgrowth of asking why it was that a polymer behaved in a certain way.

Although I was doing other things, too, I decided to settle on polymers. The polymer business was suddenly expanding, and not just industrially, but academically, as well. There were so many things that you could deal with from a physical chemical point of view that it was a fertile field. When the war broke out, I had already gotten started in polymers. It appeared that our nation had to do something about rubber, because our supply had been cut off by the Japanese. So, being interested in polymers and knowing of course the importance of rubber, I went to Roger Adams and asked if there was anything I could do that might be helpful.

He thought that was pretty reasonable, so he wrote to the U.S. Rubber Company, which had a laboratory in northern New Jersey. As a result, I got a little consulting job with them, and shortly thereafter, the government started a rubber research program.

This effort involved seeking people in the academic community to work on this program, and their representatives came to the University of Illinois and talked to Marvel. He was an organic chemist concerned with synthesis, but they wanted physical chemists, as well. Marvel came to me and said, "Fred, would you like to work on the physical chemistry end of this program?" I thought that was a pretty good idea, so I agreed to do so.

Well, then it turned out that a number of companies, including U.S. Rubber, Goodyear, Goodrich, and others, were also going to participate in this. I was going to be part-time on sponsored research, and get half my salary from this government program. Then it turned out that U.S. Rubber said that there was a question of whether or not I could get money from the government from two channels, one indirectly through U.S. Rubber and one through the research program at Illinois. They thought maybe I had to sever the consulting relationship because they'd hear about what I was doing in the meetings we'd be having. Well, I was disappointed because that was my first industrial consulting, which is a nice thing when one is young, just married, and thinking about a family and that sort of stuff.

Upon talking to over with Marvel, he said, "I'll tell you what. The DuPont Company is thinking about a need for physical chemistry." The DuPont Company was very much identified with the University of Illinois because Illinois fed them organic chemists. There was one time when three out of four directors at the Central Research Department were University of Illinois Ph.D.'s, and both Adams and Marvel were consultants. DuPont said, "Maybe we need a little physical chemistry." So Roger Adams said, "Well, we've got a fellow who's interested in polymers and can help you." It was Roger Adams who got me a consulting job with DuPont's Central Research Department, and that was the most rewarding type of consulting arrangement I've ever had in my life. I use the word rewarding in an honest sense, but it was not just financially remunerative, it was also scientifically enlightening. Monetarily speaking, it made the difference in paying off the mortgage and having a family. But more than that, it provided the opportunity for really bringing academic thinking into an industrial laboratory.

By all odds, DuPont was the most sophisticated chemical company in the country when it came to research. They understood what it meant, at that time, to think in terms of the long run without demanding an answer tomorrow. There was none of that. Their chemists would ask questions and they expected the consultants to ask searching questions, also. Ask people the right question; find out why they're doing things the way they are, and if you know an answer, give them the answer. This is the way the consulting developed, and it was a really wonderful kind of a relationship. We could talk about polymers and other things, and I could ask them why they were doing something, probing them as you might almost probe a doctoral candidate on his preliminary examination. Out of it, you could evoke responses which they didn't know they had until you asked.

Well, the unhappy part was that I spent too much time consulting. I was on the road more than I really should have been for the sake of my family. We now had the two children, two daughters, and I was all too often shuffling off to Wilmington, where the DuPont Central Research Department was. (They called it the Chemical Department then.) That's where they first discovered nylon and made the commitment for long-range chemical research. Then the Fabric and Fibers Department wanted my help, so I made a few trips down to their rayon plant in Richmond, Virginia, and up to Buffalo, where they made cellophane, and to Waynesboro, Virginia, where they had acetate rayon, and so on. Anyway, it was taking up too much of my time, so I whittled it down, and limited it pretty much to the Central Research Department.

BOHNING: You had a number of patents, three or four, didn't you?

WALL: There are a few patents. Sometimes they were an outgrowth of just talking with people about something and asking, "Why don't you try this" or "What have you done?" In at least one instance, I got a letter from a research chemist saying, "You were telling me about such-and-such. I've written up a patent application based on what you said, and I hope you will concur." I signed the application with a statement of assignment to the company. I hadn't even known I was talking about a patent in the conversation leading to the patent. I was not thinking patent, I was simply thinking science. One patent was very academic indeed. It had to do with how you could make a nylon that would resist the undesirable effects of heavy sunlight (8). It was established that something known as the alpha-hydrogens in the nylon chain were the bad actors. I said, "Well, if you can put a substituent there, you can test the hypothesis." "Yes, but it's a little hard to do." "Well," I said, "you can establish the principle as follows. Suppose you were to put deuterium (heavy hydrogen) in those alpha positions, just to test this hypothesis as to where the bad actors are. Deuterium, being twice as heavy as the hydrogens, has a different vibrational frequency, so it isn't going to be kicked off the way sunlight will kick off a hydrogen atom. Try it."

So, they made some nylon with deuterium in the alpha-hydrogen positions, and it resisted the light. There was no intention to start making nylon that way, not with heavy water. This was to establish a principle, that was the whole reason for it. But they got a patent on it. It satisfied the company scientifically and provided a forerunner to alternative substituents. Then I had a couple of others that I never imagined would lead to patents (9).

The only disappointments I had with DuPont, though, occurred when I gave them a couple of good ideas that they rejected. In retrospect, I think they were really good ideas. They had synthesized a new chromium oxide which had different stoichiometry from the normal one, and they discovered that it had some remarkable magnetic properties. This is at the time when magnetic tapes were just breaking into big business.

I said, "If it has more remarkable magnetic properties than, say, iron oxide, you've got a potential use here." "Well, it's pretty expensive. Iron oxide is cheap." I said, "I don't care if

it's expensive or not; you find out whether it's useful, and if it's useful, maybe you can make it cheap enough to be of some good." "Well, that's a thought."

So they studied the magnetic properties some more and they made some tapes out of it, which they said were better tapes than 3M was making for recording. This was well before VCRs and that sort of thing. They said, "It's better, but the trouble is, it's only about 10 percent better, and our sales department says if it's only 10 percent better, you should not tool-up for it. Not until you're twice as good do you start a new factory and get into it. But, if it's only 10 percent better, we'd be licked by 3M, who has the background, the advertising and all that."

So, they dropped it. I thought that was a mistake because I was convinced that there was going to be a future in magnetic tape. I didn't know exactly where, but then along came the business about recording TV programs. So, I brought it up again; I said, "This isn't just a matter of tape for a few computers. It's going to be a big business." So, they looked at it again. Well, this time they went to other companies and licensed it out. It's being used now, for limited purposes.

[END OF TAPE, SIDE 3]

WALL: About that time, chemistry was getting a bad name because of its effects on the environment. DuPont made a lot of DDT, and then Rachel Carson wrote a book.

BOHNING: *Silent Spring* (10)?

WALL: Yes, and I said, "You know, DuPont has always prided itself on having things that last a long time." They did indeed want to produce items that would have a long shelf life, and would last a long time. "But," I added, "It might be that there would be merit in having an insecticide that will become innocuous through deterioration in a reasonable time." They said, "Well, that violates all our principles. We're not making things to go bad; we don't want to be accused of producing shoddy things just to sell more. We want things that will persist." I said cynically, "Persist and do damage. Like DDT." Well, that didn't go over so well. The sales department didn't think that was a very smart idea. So, I said, "Well, that's my thought. I think in the long run, you're going to find that people are going to ask for insecticides that will deteriorate."

Then I made another suggestion that their public relations people did not like. I suggested that instead of their slogan, "Better Things For Better Living Through Chemistry," they make it, "Better Things For Better Living Through Research." I said, "You do research; research has a nice connotation. Chemistry is becoming a bad word. 'Better Things For Better Living Through Research' should be your slogan." Well, they sent that to their public relations office, and it was rejected. They said they had spent millions of dollars on "Better Things For

Better Living Through Chemistry” and they were not going to change that. But, do you know that some years later they changed it? Not by changing the word chemistry to research, but by dropping “Through Chemistry”. It’s now “Better Things For Better Living.”

Please understand I enjoyed my relationship with DuPont pretty much. But there was one final thing that I was disappointed with. I thought that since they had expertise in synthesizing things that they should go in for organic electrical conductors. As a physical chemist I said, “Look, electrical conductivity is important. You’re making fibers and films for various purposes. Why not make some that’ll have some physical properties useful for electronics?” Well, the old guard was a little frightened. “We are chemists, not physicists.” They acknowledged that it was interesting, but... I even went so far as to say, “I’ll bet you can make a polymer that, when drawn out as a fiber, will conduct in the longitudinal direction of the fiber, but not transversely, so it’ll be its own insulator.” They said, “Well, that’s science fiction.” I do not know if anyone has made such a polymer yet, but the point is, organic semi-conductors and organic conductors are very much the vogue now. DuPont thought chemically, but was fearful of physics. They knew that synthetic chemistry was so valuable that they were going to stick with that with which they were comfortable. So they shied away from it. However, in the last few years, they’ve gotten into it, not necessarily as I might have suggested.

Well, my relationship with DuPont was ultimately terminated voluntarily on my part. When we moved out to California, I found that the travel was just too much. I worked pretty hard for DuPont. If I flew there one day before consulting with a time change of three hours, then after a couple of days flew back home, it was pretty rough on me, especially when I had administrative responsibilities here. So I reluctantly resigned as a consultant; they were very nice about it because they paid me a final unearned honorarium. Indeed, on one later occasion I went and talked to them on a purely ad hoc basis and they were very receptive and gave me an honorarium for that. My consulting at DuPont was by and large a good combination of things. Except for the fact that I traveled too much, it was a rewarding experience.

BOHNING: Did you do any other consulting?

WALL: Well, subsequently.

BOHNING: But not while you were with DuPont?

WALL: No. I had been asked to consult with another company, but I declined; I told them I was already consulting with a competitor.

However, I did give some expert testimony in a court trial for 3M. This did not conflict with DuPont because it had absolutely nothing to do with them. It had to do with interpretation of laboratory results. I was in at least two trials, and a third was scheduled but subsequently

canceled. The first one went over very well and the judge made a nice remark about my testimony. Then 3M had another case, which I studied and said, "I don't think you've got a good case." They said, "We must go to court and have it tried." I agreed to testify, but I did not think the case was as good as the first. Sure enough, they lost. It didn't make me feel good because they paid me for testifying.

BOHNING: I understand.

WALL: That bothered me. Then they had a third case, in which I thought they were on the winning side, but it turned out that although a trial was scheduled, the case was dropped because of an out-of-court settlement. I don't know what the settlement was. That is the extent of such expert testimony. This occurred while I was still at Illinois.

BOHNING: When you were at U.S. Rubber, was Frank Mayo there?

WALL: Yes, he was.

BOHNING: Did you interact with him?

WALL: Yes, I did.

BOHNING: Because later on, I noticed you had some collaboration.

WALL: Oh, it was just a little thing. Different people around the country were working on similar problems but using different notations; the idea was to agree on some notation. I met Frank Mayo at U.S. Rubber and was very much impressed by him. He was a sound individual. He subsequently went to Stanford Research Institute. Mayo is a very capable fellow.

Well, perhaps I should turn to Illinois and tell about some of my experiences there.

BOHNING: Yes.

WALL: I got involved in a fair amount of committee work, the kind of thing that you get loaded with if you don't protest too much. I worked on several committees, one of which involved planning for the future. I got to know some of the people on that committee. Indeed, I made an

excellent friendship with a civil engineer, a fellow by the name of Nathan Newmark, who was also on the committee. This turned out to be a fine friendship—our families became involved, and we maintained connections right along.

My committee work was evidently noted because then I was made a member of the University Research Board, which was an adjunct of the graduate school. The dean was chairman of the University Research Board. It was good experience because I acquired a feel for things going on in the university, outside of my own field. We'd have requests from people in the humanities, social sciences, and so on, and all this was broadening. I really did like the fact that there was something more than just chemistry; as much as my capacities in research were limited to chemistry, I still enjoyed learning about other things and finding out what was going on.

The dean, whose name was [Robert] Carmichael, ultimately retired and was followed by a new dean, Louis Ridenour, a physicist. Well, Louis Ridenour took leave one year to work in the Pentagon in connection with some research for the Korean War. At that time, they appointed an acting dean, but Louis suggested that maybe they should have someone other than the acting dean serve as chairman of the Research Board. They put the finger on me, making me chairman of the Research Board while there was an acting dean for the routine of the graduate school. It turned out that the finances were somewhat in a shambles and that Louis had made commitments that exceeded the amount of funds available. I had to make special presentations to the top administration of the university to bail us out and get us back on track. Although it was an unhappy task to have to straighten things out, it turned out well for me because that was noted and the administration took kindly to me for what I had done. So, for the ensuing year I was asked to be acting dean.

The first acting dean had quit after one year, and the president of the university said to me, "Since you still want your connection to chemistry, you probably don't want to have all the graduate school duties, so we'll relieve you of the chairmanship of the Research Board while you're acting dean." The deanship had a titular prescription; the chairman of the Research Board did not. I said, "The chairmanship of the Research Board is more interesting than being dean, I'm sure. If you want me to be acting dean, I'll do it, and I'll do what the dean used to do. But I also want to be chairman of the Research Board, and if I have to make a choice I'll take the board and not the deanship." He said, "All right, if that's the way you want it, do it." So I became acting dean.

That put me at a crossroads. The question was whether I could continue any research while I had administrative duties. I said to myself, "This is a temporary job. What happens when I go back?" I had to keep on with my research, which I did. I didn't do any teaching then, but instead of serving on committees, I appointed committees, which helped. I worked hard, but I didn't forego my ongoing research.

At that particular time, the University of Illinois was just getting started in some computer work. Thanks to Louis Ridenour, who was interested in developing computers, Illinois got a start on an ad hoc basis. I then worked hard to get a permanent budget item for a

computer laboratory. It wasn't a large item to start with, but it was a listed item, not just ad hoc. I was very interested in the computer and wanted to see it developed. Ultimately, they built what is known as the ILLIAC, which was based on John von Neumann's design for Princeton, but which was actually completed at Illinois before they completed the one at Princeton. I don't know if you've ever heard about the ILLIAC, but it was one of the first university high-speed digital computers—high speed for those days, low by any standards today.

BOHNING: What year was this?

WALL: Oh, this was after the war, along about 1950 or thereabouts.

After my year as acting dean, I was asked if I wanted to be permanent dean. I declined, so they had to look for a permanent dean. I let the president know that and I said, "I am still interested in helping the university. If you don't mind, I would be delighted to continue as chairman of the Research Board." I didn't fully know at the time why this was, other than the fact that it was fun learning about the different things going on and having a hand in supporting research. If you did the job well, the people around the rest of the university began to like you for it, so it was doubly rewarding.

I continued as chairman of the Research Board and had a good rapport with the new permanent dean, a botanist by the name of Oswald Tippo. Then he left and ultimately went to Colorado. Again, they made a search for a dean and I was asked again to be dean. This time I said, "All right." I had already done enough to know that I could continue to do research under such circumstances, so I felt that I could as dean. I'm reminded of the old story of a husky fellow who could carry a cow in his arms, and people asked how he could do it. Well, he started with a calf and every day, he carried the same calf. The calf got bigger, but he never stopped carrying it, and that's why he could still carry the cow. Well, in a way, that was it. I was able to do research and carry on the other duties, and my best research was done while I was dean. It was while I was dean that I did the research that presumably got me elected to the National Academy of Sciences.

At any rate, I was then the permanent dean and chairman of the Research Board, and we got a new president at the same time. My research was going great guns, and part of the reason was that I became involved in computer calculations. I was interested in the computer, I was interested in statistics, and the ILLIAC had been completed. Specifically, I was interested in configurations of macromolecules, rubber-like molecules, so we started a Monte Carlo method for simulating configurations of coiling-type molecules. There's a paper on it co-authored with [L. A.] Hiller and [David J.] Wheeler (7). That, by the way, was the first paper on Monte Carlo simulation of macromolecules generated on a high-speed digital computer. It was just prior to that that the computer laboratory had been formally established. I got them a budget as dean and kept pushing.

BOHNING: Wasn't Wheeler in the computing laboratory?

WALL: Yes, but he was a visitor from England, actually from Oxford [University]. You see, Illinois had already gained a reputation for being a university with a strong interest in computers. There weren't too many around. This was early on. So this fellow from Oxford came to the University of Illinois for a year, during which time he, Hiller, and I worked on the Monte Carlo technique for macromolecule simulation.

BOHNING: Was Hiller one of your graduate students?

WALL: No. There's an interesting story about Hiller. I'll tell you about him, too.

BOHNING: Okay.

WALL: Wheeler, Hiller and I got together on the problem. Hiller and Wheeler prepared the programming for the machine, a time-consuming business because in those days you had to write in machine language. It was really a chore.

BOHNING: Yes.

WALL: So, they worked up a program for the Monte Carlo simulation of polymers. It was a breakthrough.

BOHNING: Is this the one where you used the vectors?

WALL: Well, the machine was programmed to take random steps on a tetrahedral lattice. After each step, there would be three choices for the next step, and so on. But then we had to be sure that we never occupied the same place twice. In other words, we generated self-avoiding random walks. There was significant sample attrition, which we kept working on.

We did a fair amount of additional work on that, but then I did something else also with Hiller and [J.] Mazur (11). This was the calculation of the dynamics of chemical reactions, "Statistical Computation of Reaction Probabilities." That, again, was the first computer calculation of any serious magnitude for calculating collision probabilities. We took a simple case. A hydrogen molecule was hit by a hydrogen atom. What's the chance that this incoming atom will kick off one of the molecular atoms? That is the simplest possible chemical reaction

you can think of. Of course, one must start with rudimentary things. Then we did some more on it, and happily John Polanyi, of Toronto, said in his Nobel Prize address that there were lots of things done with computers—the full impact of the method did not come up until Wall and his colleagues did their work. I think you'll find that in Polanyi's paper (12). I was pleased that he said it.

BOHNING: Well, that's quite a recognition.

WALL: Yes, but by the same token, I have to tell you something else. I was never one to follow through enough to be identified as one who could be called the old man of that field. I would do something and when I was satisfied that it could be done, I'd say, "Isn't there something else I could do?" The reaction probability business has burgeoned. People are calculating such stuff all around the country and around the world.

BOHNING: Yes.

WALL: It's going on all the time, with a lot faster computers than the original ILLIAC.

Now, my stay at Illinois was overall a pretty happy one, except for one thing. I'm telling you the truth about things now. I didn't get along well with the new university president. This president had not been involved in my appointments. He was very much concerned with public relations, so much so that he appeared to regard public relations as an end in itself. Maybe I'm exaggerating, but he seemed to feel that if you could persuade people that you were good, that was all you had to do; it was not necessary to actually be good. I felt that public relations may be a means to some end, but instead of bragging about what we're doing, we should just do it and let someone else talk about it. I said to the president, "My goal would be to make the University of Illinois the best university between Harvard and [University of California] Berkeley, and I think we can achieve that in our lifetimes." He agreed that was a worthy goal, but we did not agree on how it might be achieved.

This gets me back to Linus Pauling. There is a fund established at Illinois, a so-called George A. Miller Fund, used for lectureships and various other purposes. The chemistry department wanted to invite Linus Pauling to give some lectures, so I was asked by the department to extend the invitation. So I wrote to Linus asking if he would come and give some lectures. He thought the thing over and wrote back and said he would like to do that. This correspondence took place in the spring of the year and planned for lectures to be given some time during the ensuing academic year. We agreed and set up a mutually satisfactory schedule.

[END OF TAPE, SIDE 4]

WALL: However, during the summer, the legislature of the state of Illinois passed a law saying that no state funds could be expended to pay the salaries of any person who would not sign a non-Communist oath. You see what I'm getting to.

BOHNING: Yes.

WALL: I saw an immediate loophole; I was pretty sure this was a legitimate loophole. I said, "These aren't state funds; the Miller funds are trust funds." The president said, "Oh, no. We have to treat them the same as if they're state funds. That is our responsibility." I said, "I shall write to Pauling and see if he will sign the non-Communist oath." So, I wrote him a letter. I told him that during the summer the legislature had passed such and such a law, and that if he were to be paid, he'd be obliged to sign a non-Communist oath. I was sorry about this, knowing how he felt about matters of this kind, but I hoped, of course, that he would come and give us the lectures.

Well, I didn't hear from Linus. I was a little leery about pressing him on this matter, but it turned out that the Association of Graduate Schools, which is made up of graduate deans, was going to have a meeting in L.A., with one day in Pasadena. So I wrote a letter to Linus. I said that I expected to be out in Pasadena at such-and-such a time and added, "I wonder if I could drop in to see you." I didn't say anything about the oath, just, "Could I drop in and see you?" Immediately I got a letter back saying, more or less, "Be delighted to see you, Fred. Look forward to seeing you at such-and-such a time."

So, I got out to the meeting and went to Linus's office at the appointed hour and entered the room. It started, "Hello, Fred," and "Hello, Linus." Well, almost before I could get seated—I might have still been standing up, I don't recall—when he said, "Fred, I decided I will sign." You could have knocked me over with a feather. I sat down, and we talked a bit. He said, "Fred, you understand, I'm not a citizen of the state of Illinois. I'm a citizen of the state of California. I feel that I cannot really protest what goes on in Illinois, but I would not do this in California." That's what he said. I said, "Well, Linus, I can appreciate your feelings. We will be delighted to have you."

I did not ask for him to hand me the slip right then and there. He voluntarily said, "I will be sending you the form." So in about a week or ten days, I received the form in the mail, duly signed and notarized. I brought it to our business office, deposited it there, and let the public relations people announce that Linus Pauling was going to give lectures at the University of Illinois. Everything was now in the works. He had fulfilled the requirements. Okay, the notice goes out and the press reports it. Immediately the American Legion said, "You mean you're going to have that Communist come and give lectures at the University of Illinois?" We said, "Look, he signed the non-Communist oath." They said, "That doesn't mean anything. Communists will sign the oath because you can't trust them," which reminded us of the arguments that went on when the debate was going on in the legislature. Some people said,

“This oath won’t mean anything because Communists, if they’re liars, will sign the oath anyhow.” “Oh, but it’ll protect us,” said those who wanted the law. At any rate, the American Legion objected, but the deal was on.

I wrote a letter to Linus saying, “You may have heard indirectly about these objections, but don’t worry about it. They protested the Girl Scouts because they had somebody who favored internationalism meet with them.” I decided I’d let Linus know rather than let him hear about it otherwise. So he came and gave the lectures and they were very well received. In fact, for the first lecture there was standing room only, and we didn’t meet in a regular chemistry lecture room; we went to the music hall. He got a tremendous crowd. Then he gave three lectures in chemistry; some students were obliged to sit on the steps. Then he gave another one in the music hall; this time everybody had seating. Then he gave one over in physics, and a couple of others.

Well, at any rate, these lectures were well received and that was very nice. Considering how Linus felt about oaths, et cetera, how we got him, I don’t know.

BOHNING: That’s a historic moment, I would think, to have him sign that.

WALL: Well, don’t you think that was remarkable? I was a little amazed at how Pauling handled it.

BOHNING: What year was that?

WALL: Let’s see here. It was 1956 or maybe early 1957 that he gave the lectures.

BOHNING: All right.

WALL: I cite this business about Pauling because it’s more than an anecdote about him.

BOHNING: Oh, no, that’s fascinating.

WALL: It also disclosed that the university president was exceedingly sensitive to this. He kind of wished that Pauling hadn’t shown up. In fact, there were occasions—social occasions, dinners, one thing or another—that the president never attended. He didn’t want to be seen in the same room with him. That’s the kind of thing. Well, then another thing happened that relates to it.

In the course of my tenure as dean, there was a graduate student in mechanical engineering by the name of [Edward] Yellin who had refused to testify before the House Un-American Activities Committee. He was prosecuted and found guilty of something, but I don't remember the precise thing. It related to the business of alleged Communist activity and failure to testify. At any rate, he appealed and his case ultimately went to the Supreme Court at a time when [Earl] Warren was Chief Justice. The Supreme Court threw out the business. While this was going on, and prior to the Supreme Court decision, it was said that we should throw him out of the university. Now, he was a graduate student, and responsibility for graduate student affairs was the business of the dean. I was anxious to be a house mother, so to speak, to graduate students. Still, I resented what was going on when I read in the newspapers that the administration was seriously thinking about throwing Yellin out of school. I made it clear that if anybody was going to throw him out of school, I was the one to do it. Not that I wanted to throw him out of school, but I didn't want somebody else to step in and encroach on my responsibilities.

Well, the heat was on, but that same time, I was scheduled to give lectures out in California—at Caltech, UCLA, and Berkeley. I was going to give three lectures. This had been well-scheduled, and it was coming at a time when the Yellin thing was reaching a head. I realized that if I left town, the president would almost surely find some way to throw the guy out during my absence. The question was what I should do. What I did was to compromise in a sense. I suspended Yellin. I didn't throw him out, but I suspended him pending investigation. That took the immediate heat off because no one could say I had done nothing. Of course, there was a lot of criticism of me for suspending him. The people who were liberally minded said, "What did you do that for? Stand up for his rights." Others would say, "Well, at least that was a step in the right direction."

At that time, I had two associate deans. One handled general student affairs, and the other handled the research end of things. While I was gone, the Associate Dean handling student affairs appointed a committee to make recommendations as to the disposition of the case. It was a small committee, not more than five. One of them was John Bardeen, a highly distinguished physicist. The committee recommended that Yellin be reinstated. This all happened while I was on the trip, and when I returned, I saw and accepted the recommendation. I talked to the then-provost and vice president, Gordon Ray (who subsequently went to the Guggenheim Foundation). He asked, "Are you going to accept the recommendation?" I said, "Yes." He said, "Okay, Fred, but you know, the president isn't going to like this." I said, "Well, it's the recommendation of a faculty committee. I concur with this recommendation, and I shall reinstate him." So he was reinstated.

I did not come out looking very good to many liberals. They said I was wishy-washy, but I can now assert that Yellin wouldn't have had the chance of a snowball in hell when I was gone, because I knew enough about how the university functioned. I put him, you might say, in protective custody. That's what it really amounted to. Anyway, he continued his legal appeal and was sustained by the Supreme Court. Subsequently, I talked with him. He was grateful for the fact that I had reinstated him. I doubt that he was pleased that I had suspended him, but I

told him as much as I reasonably could. A year or two later, after my resignation from the deanship was announced, he spoke to me and asked if he was responsible for my leaving. I sidestepped that question.

All of the foregoing contributed to the dissatisfaction of the president with me. Then one or two years later, there was a general university budget increase with widespread salary increases, so that practically everybody got a salary increase. The president got a salary increase, so did the vice presidents, the faculty in general, and practically every administrator except me. I felt that I had not only conducted the affairs of the office satisfactorily, but during the preceding year had been elected to the National Academy of Sciences and made president-elect of the Association of Graduate Schools. The message was clear.

From time to time, I had heard about jobs. I had been offered a number of positions in various places while I was dean. I had even been approached about a couple of minor university presidencies, but they were not attractive to me. I would thank people for considering me, but nevertheless decline. When you come right down to it, I was never cut out to be a university president anyway. If I had been offered a real good one I might have accepted, but it wouldn't have been good for me or the university involved because I'm lousy at public relations, and I'm terrible when it comes to money-raising. You couldn't have convinced me of that in my younger days, because I really thought I had sufficient capabilities, but when I look back, I think I'd have been a dismal failure as a college president. I could be a second in command; I could be a dean, but when it comes to hobnobbing with people to give you money, I'd have been an absolute wash-out. I think I can honestly be critical of myself on that score.

Well, I had a couple of offers right at that time, but I had a hankering to go out to California, and I did get a job offer from Santa Barbara. Santa Barbara would not have been my first choice; I'd heard about La Jolla getting started, but never made any contact with them. Of course, I'd have preferred Berkeley if they had been willing to take me, but no offers were coming from Berkeley. But, I thought, okay, Santa Barbara is an on-the-make university; California is growing. There's a future, that sort of thing. So I did go to Santa Barbara. I was in chemistry, and I was also made the Vice Chancellor for Research. Chancellor Cheadle wanted me to stay there, but by then I'd made contact with La Jolla. I felt kind of guilty about leaving Santa Barbara that soon, but I did come to La Jolla, where I felt that the potential was greater, and I was made Vice Chancellor of Graduate Studies and Research.

Then I learned something that I would never have anticipated. At Illinois, I was successful as a dean in the eyes of the faculty and my peers, notwithstanding the president. They thought I had done a good job. I said, "Well, if I could do it there, I could do it here." But I overlooked one thing. I had grown up at Illinois; the people knew me. They trusted me. I wasn't out to skin them. In California, many people assumed the administration was out to screw the faculty. That was the attitude. You could not really be an administrator and be trusted by people coming in from all over the country. UCSD had an excellent bunch of people. They were not intellectual ragamuffins, quite the contrary. They were top-notch people who were dissatisfied with where they had been, often distrustful of whatever establishments they came from. It was very difficult to really carry on a sound administrative program because so many

wanted to be their own bosses and had the credentials. That was UCSD at the start. What was a rather glorious period at Illinois—except for the relationship to the top—was not equally pleasant at UCSD because the situation was reversed. The chancellor thought I was a nice guy; the faculty had to be persuaded that I wasn't out to beat them.

BOHNING: It appears to be a thankless job.

WALL: It was a real turnabout. Incidentally, I told you about Linus Pauling and Illinois. Let me tell you some more, because Linus got into my life several times.

When I indicated that I was going to Santa Barbara, Pauling had just moved, or was going to move, to Santa Barbara to be at Hutchin's Institute [Center for the Study of Democratic Institutions]. I forget the precise name for it, but it was related to international affairs. He heard I was going to Santa Barbara, and he wanted to know if he might have an adjunct appointment, have a student or two and so on, some kind of nominal relationship. I said, "That would be great." I thought, it would be wonderful. When I move to the new location, we'll start off with a bang. We're going to have a distinguished person associated with it. Indeed, I saw great things.

I was not yet on the scene. I was to come to Santa Barbara, but I wasn't yet on the faculty, so all I could do was make a recommendation. I could not initiate papers or anything like that, but I wrote to the department at UCSB and said that Professor Pauling would like to have a nominal appointment so that he could have graduate students work with him while he's working at Hutchin's Institute. Before any papers were officially sent through, the chancellor was asked about it and it was indicated that this recommendation would be forthcoming. The chancellor immediately went to the president, Clark Kerr, who I think wouldn't personally be against Linus Pauling, but who was mindful of how the regents would behave. There were a couple of regents who objected; at least one who allegedly said, "Over my dead body. We're not going to have Pauling associated with the University of California in any way, shape, or form." So, the chancellor said, "It can't be done."

I don't remember now whether I told Linus that there had been an inquiry made and there was some problem with the regents, but at any rate, he got word of it and got in touch with the chancellor. Evidently, he had a pretty rough affair with the chancellor, to the point where they would not talk to each other. The upshot was that Linus was pretty sore, and for a while I thought he might be sore at me because he thought I was supposed to send through forms. I told him that I couldn't send through any forms; I could only make a recommendation. Other people had to send in the forms, but I certainly did make the inquiry. He forgave me, but he was still miffed. Subsequently, when I saw him, he said, "Well, Fred, I don't hold this against you, but I will hold it against Santa Barbara, and I'm never going to set foot on that campus." That's what he told me.

Some time during the next year when I was on the campus, a student-body organization wanted Pauling to give a speech to them on campus. I first heard about it when they came to me and asked, "You know him, don't you?" I said, "I do." "Well, will you introduce him?" I said, "Sure, I'll be glad to." So sure enough, on the appointed day, Linus and I each appeared at the appropriate place, and I introduced him from the speaker's platform. It was a very nice talk and there was a discussion afterwards and many students asked questions. He's a wonderful speaker, you know, and when he really gets going, he can excite the students. Finally, I had to terminate the session. I said, "Sorry, but we really do have to bring this to a close."

When I talked with Linus briefly afterwards, I said, "That was a very fine lecture. We're glad you were able to come." Then, recalling what he had said to me earlier, said, "You know, Fred, I told you I was never going to set foot on this campus. What I meant was, I wasn't going to come if the administration had anything to do with it, but these were students." [laughter] So, I remember, "I'm not a citizen of Illinois. I wouldn't do this in California."

BOHNING: These anecdotes are very important.

WALL: I don't know who else could tell them. I have one more Linus Pauling story coming up. [laughing] This is going to be more about Pauling than me by the time we're through.

[END OF TAPE, SIDE 5]

WALL: I was in La Jolla, and Linus was getting fed up with the Hutchin's Institute in Santa Barbara. In fact, he once told me that all they did was talk and they never did any work. He spoke very disparagingly about the fact that they had these lofty goals but were just fooling around. So, somebody in La Jolla, I think the physics people, thought maybe it'd be nice to get Linus to come to UCSD. John Galbraith, the chancellor at UCSD—no relative of the economist John Galbraith—asked me what I knew about it. I said, "Well, I think it'd be great if you could get him, but let me tell you about Santa Barbara. Their regents said they wouldn't have anything to do with him." John Galbraith kind of shook his head and said, "Well, I don't know how we can manage this."

Then it occurred to me that there was one thing that could be done. He could be appointed at a level that didn't call for regental purview. Back in the early days, the regents presumably appointed anybody who worked at the university. Subsequently, when the university got bigger, they said, "Well, we'll deal only with professors or tenured people, and not bother with the others." Then they said, "Well, we'll only take care of professors, deans, and so on." Then they said, "Well, we won't even bother with in scale professors. The chancellors can take care of those, unless they're over-scale professors." The over-scale had to do with exceeding the rather rigid levels for salaries: assistant professors I, II, III, et cetera, all the way

up to professor I, II, et cetera. Above them is the continuum over-scale, and regents retained their prerogative to exercise purview for all over-scale appointments.

In terms of reputation, Pauling would have qualified for an over-scale appointment, but that would have meant regental review. How does one get around that? Simple: appoint him at the highest possible level that doesn't call for regental purview. That would be at the top research scientist level, which is pegged at the professorial level, except they were not called professors; they were called research scientists, or research associates. Pauling was put on an eleven-month appointment, so that there'd be some allowance for summer remuneration, and he was to be paid for out of grants from government agencies. Pauling was duly appointed, and the regents never officially saw the papers, although they might have heard about it.

I also remember that at Santa Barbara, he was honored by the Romanian Embassy, for which they arranged a reception for him, to be held at the Institute. Pauling asked that Clara and I be invited to the occasion, which we attended. There weren't many people there. Subsequently, when we were in Washington, we were invited to the Russian Embassy, where Pauling was getting a Lenin Award. I must say the Russian staff in the embassy were less than diplomatic. When they were inviting me, and they said something like, "Professor Wall?" "Yes." "You know, there's a Linus Pauling who's getting a Lenin Medal, and he thought it would be a good idea if we asked you and your wife to come." We went, but there were very few people there other than embassy personnel. There was something funny about the photography. Somebody was taking pictures, but they were careful never to have both Russians and U.S. citizens in the same picture. I could guess why, but I really did not know.

BOHNING: I'd like to come back at some point and talk about some of the research you did at Illinois. While you were dean at Illinois, you maintained your research level pretty well.

WALL: Yes, I did.

BOHNING: But that sort of stopped when you came out here.

WALL: It did. For the reason I gave you about the guy who could carry the cow. I grew up at Illinois and I didn't have to fight the faculty. They trusted me, for I grew up with them. That makes all the difference in the world. If you come to a place where administrators are not trusted, the atmosphere is totally different.

BOHNING: How did you feel during that five-year period—at Santa Barbara and then down here—about not doing any research? You'd been really active right up to that point.

WALL: That was kind of demoralizing. This was something I didn't anticipate. I didn't reckon with the possibility that I couldn't resume research. I could not do two jobs effectively without growing with them together, as I did in Illinois. I got along pretty well, but not nearly as well as I'd have liked. Then I was offered a job at the American Chemical Society, to be executive director.

BOHNING: How did that come about?

WALL: Well, that is because the chairman of the board there knew me and thought that I could manage the place and they needed somebody to do so. It turned out that there was a job to be done; unhappily, the circumstances were such that I couldn't achieve what I wanted to accomplish. It was really a disaster; I returned to academic work. I went to Rice University, and there I was just a plain professor again. Although I was later asked to head up the department, I absolutely didn't do it. I did resume research and Rice was a fine place for it. They don't overwork you at teaching. I immediately got a grant from the Welch Foundation, which enabled me to get started, and there was evidence that research was appreciated.

BOHNING: Well, you came right back. You had a paper on rubber-like elasticity (13) and a paper on the Monte Carlo methods (14), and you were right back in it again pretty quickly.

WALL: I went back to polymer work for two interesting reasons that I shall explain. I thought about what kind of research I should do. I liked Monte Carlo techniques, but should I go back to polymer configurations? Or should I go back to chemical reactions? Well, I discounted the latter for a very practical reason. There had been so much done about that in the intervening years by people who had great expertise in computers, using really high-speed digital computers, that it would have taken all my effort to catch up.

But there was another point. I could catch up on polymer configurations, but I was also interested in something else that actually goes back to something Richard Tolman once told me. It was at the end of my year at Caltech and I went in and talked with Tolman to ask how I did in his course. He said very well and that I got an A in the course. He told me about some of the things he was doing, and he said, "You know, I'm looking into the business of extending the uncertainty principle to an absolute limit. We say you can't simultaneously measure both coordinates and moments with unlimited precision. If you measure one precisely, you don't know the other, and vice versa." Tolman said he wanted to look into the absolute business of the uncertainty principle, and wanted to inquire into whether or not there is a smallest distance less than which it's meaningless to talk about things irrespective of how little you know about the momentum. In other words, is there a distance less than which you shouldn't even imagine? Do we have granular space? That had been talked about by various people.

That thought appealed to me—granular space. I began to think about lattices, and I began to think about lattice representations for solving quantum mechanical problems—not as approximations to solving differential equations, but maybe to give an insight into something deeper. People use computers to solve differential equations by use of finite difference equations on a fine grid. If the grid is fine enough, one might get a good representation for what is presumed to be a continuous function. But suppose your fine-enough grid were as fine as it could be in the light of some principle, and that there could be nothing finer. Would you then not have the solution? I'm not saying that space must be made of little cubes. Let's not worry about the shape, but if there are certain ultimate modules into which particles might fit, what would the mechanics be?

That was a thought that I kept in mind. I wanted to explore discrete systems, but pragmatist that I was, how could I get support? Should I go to the NSF [National Science Foundation] and ask for a grant so that I can figure out discrete mechanics? If I did, I would get shooed out in an instant. But I had been generating configurations, random configurations, on lattices. They were discrete systems. A lot of other people would say that the way to handle polymers is to simulate continuity. I avoided that because I wanted to learn more and more about lattice systems. So, I studied matrix representations of things more precisely. I started thinking about eigen values of vectors rather than eigen values associated with differential equations. Therefore, I decided to work on polymer configurations on lattices. I can write to the Welch Foundation and say, "I am interested in macromolecular configurations" and they'll say, "That is good chemistry." To a certain extent, that was a cover story. I sound devious, don't I? [laughter]

BOHNING: No.

WALL: Well, it was a kind of a cover. I say devious because, obviously, if one were involved in espionage, he'd have to appear to be gainfully employed in some way—like running a shoe store or a book shop. I had a hankering to learn more about discrete systems, so back to macromolecular configurations on lattices. I began to do more with matrices. I began to do more in finite space. Then, instead of continuous orthogonal functions, I got to thinking about matrices and orthogonal vectors, and how to work with them. This was a prelude to my post-retirement publications. Do you have the last ones there where I talked about discrete mechanics?

BOHNING: Yes.

WALL: Well, they have not caused any stir, but anyhow that's what I was working on, and it was fun.

BOHNING: Yes. Through 1988, there are five papers on discrete wave mechanics (15).

WALL: Okay. This was an outgrowth of thinking in terms of discrete systems. For example, I handled the hydrogen atom using discrete matrix representations, I was able to get the Bohr formula for the energy levels, and I developed a set of discrete vectors and components of vectors instead of polynomials—in an orthogonal set that would be the counterpart of the associated Languere functions. Then I also worked on the simple harmonic oscillator, using discrete coin tossing probabilities instead of the Gaussian. Then I discovered that I was not the first to do this mathematically, even though I had a different application in mind.

One might say it would have been quicker had I looked it up. Where was I going to look? I did not know where to start. I worked hard on it, but then all I can say is that I learned more about it than I would if I had looked it up. This experience gave me an understanding as to what was going on so that I was able to proceed with more confidence. What I miss, unhappily, is that I can't wander down the hall and drop in on a colleague and say, "I've got a thought; what do you think of it?" Now I walk down the hall and I go by our laundry room.

BOHNING: Well, do you spend any time over at UCSD? Do you still have an adjunct appointment?

WALL: Well, I've had an adjunct appointment, but that's terminating actually at the end of this month. I've got another week or so to go, and it doesn't make much difference; I still have privileges as a visiting scientist, as they might call me. I'm a retiree, so I have a few perquisites. I can use the library and all that sort of thing. I hold an adjunct professorship because I'm not officially emeritus at UCSD. I'm emeritus at Illinois by special prescription and emeritus at Rice, so I'm twice an emeritus professor, but not here because I wasn't here for enough years. I was at Illinois long enough, so they said, "We'll give you emeritus status by special dispensation." I retired from Rice after I reached the required age, so I became emeritus.

The adjunct appointment came out because of something else, unrelated to my science. We wanted to move back to La Jolla, build this house and so on, so I taught on a year-to-year basis at San Diego State University, which was also an interesting experience. People might say, "What did you do that for?" Well, I didn't want to get away from academia. I like to teach and be associated with academia. Besides, I could do theoretical research here at home even if I were teaching at San Diego State. But after three years, I lost interest in SDSU. They were nice to me, but I felt it was better to give way to some of their young people. So I resigned.

BOHNING: What were you teaching there? Physical chemistry?

WALL: Physical chemistry. They wanted me to stay on as an adjunct, but I didn't. My adjunct position at UCSD resulted from talking to Herb York, who was running an institute on science and public affairs, which conducted a seminar on U.S. foreign policy with particular reference to arms control. I thought that was an interesting subject. I'd become interested in social sciences and its interactions with science. (My wife's a political scientist.)

Herb York asked me to assist in his course on U.S. foreign policy. I didn't know much about it, and I still don't know much about it, but I was willing to study it. The activities involved a seminar, not a regular course. Initially, I just helped Herb and handled much of the machinery of the seminar. We gathered outside speakers, made assignments, called on students to give reports, and things like that. We would talk about certain books that were required reading. All that was the reason for my adjunct appointment; in two or three years, I was actually conducting the seminar, grading students, et cetera. But then Herb York retired and the role of the institute changed. A new director was appointed and the seminar dropped.

Talking to political scientists—and most of the students were political scientists—is different from talking to chemists. Political scientists will argue with you about anything. There's always somebody who disagrees with whatever. I don't know if I mentioned it before, but one of my favorite subjects to teach is thermodynamics. I had no objections to students asking questions or disputing something, but when it occurs, one tries to explain objectively. Well, it was different in the institute because so much is subjective. My experience was most interesting, and you now know why I was an adjunct at the University of California at San Diego.

[END OF TAPE, SIDE 6]

BOHNING: I wanted to ask you about the book on chemical thermodynamics, which went through three editions (16). That was where I first encountered your name a long time ago, through that book on thermodynamics. There have always been many books written on thermodynamics. Why did you write one?

WALL: I'll tell you why. Thermodynamics appealed to me as something that can be handled in a very logical way, almost like Euclidian geometry. You had postulates, the first law, the second law. Of course, there are different opinions about how best to treat the subject, but it does require a formal development. You deduce things that are beautiful to carry out, yet with enough diversity to give flavor. Well, I found something wrong with every book. Of course, there's something wrong in my book, too, but still I thought that maybe I could make some kind of a contribution by writing a book. So I wrote a book for a series published by Freeman; Pauling was, so to speak, the editor of this series. It was hard work, really, because I tend to be a perfectionist, and I was never satisfied with it. I'm still not satisfied with it.

BOHNING: That's the series by Pauling.

WALL: Yes.

I did it because the subject was fun to teach. The mathematics isn't hard—it's not as hard as that required for quantum mechanics or relativity—but it does require you to make sure you know what you're talking about. Incidentally, Tolman could really talk about thermodynamics in a wonderful way. He really was a marvel at that sort of thing. His book, *Relativity, Thermodynamics and Cosmology*, reflected his way of thinking about things (17). He put his heart and soul into the business. I remember a final examination that he gave. He asked some questions on thermodynamics even in his course on special relativity. I think he threw such items in because he liked the subject; I did too, as it turned out.

BOHNING: Well, you have macromolecules in the second edition.

WALL: Yes, I have a little bit about that. In a way, you might say, it's patchwork. I hope one can be forgiven for throwing in a bit of personal bias here and there.

BOHNING: Why not? You're writing the book. You should.

WALL: Yes, I guess one has that right. I enjoyed teaching thermodynamics, and I especially enjoyed it because of the form that it could assume even in its examinations. I conducted my examinations almost entirely as open-book exams. A student could bring in anything he wanted: notes, books, anything from the library. The only rule was that you could not talk with anybody else while writing the examination. I think that is the best way to conduct an exam, and I'll tell you why. It tests the capacity to put things together. When a person is gainfully employed, there's nothing to preclude him from taking a book off the library shelf and refreshing his memory. "Oh, I remember this. Professor so-and-so told me about how to work such a problem. Now I remember what he told me."

In preparing an open-book exam, it would be a mistake to ask a question if the answer is forthcoming just by substituting into a single formula found in the book; that would be absurd. What you need is a two or three step problem. Take formula twenty-three and combine it with equation eighteen, say, and then go on to twenty-seven and carry it to the answer. It's all in the book, but you have to know how to assemble the sequence. You can go out in the world and make the same calculations. A sequence—it doesn't have to be complex, sometimes just two steps—makes a good question. It can be made trickier by seeing that one of the things that comes out of the first step is a parameter that cancels later. That is a little tricky, but a good student proceeds with confidence. A poor student might say, "I don't have enough information to solve this."

The student who knows his subject breezes through an open-book exam. The student who doesn't know it really flubs it, and the spread of grades is broadened. Before the first exam in any term, I would tell students, "Now, it's going to be an open-book exam. Some of you are going to think you don't have to study. It always happens that way. But, believe me, if you don't believe me now, you're going to know after the first couple of exams that it pays to study." Unhappily, some students said they had no incentive to study. They knew they could use their books.

BOHNING: Yes. Oh, I've had it happen.

WALL: I remember a gem of a question I once used. Our textbook had an error in one of its equations. (This was not my book, which is not to say mine is error free.) In my exam, I asserted that equation so-and-so on page such-and-such of the text was in error. What is the right answer? The good students figured it out, and gave the right answer. One of the less able students said, "It looks right to me." [laughter] If it was in print, he thought it must be right. I am proud of that kind of examination. Such questions elicit thought and analysis.

BOHNING: Oh, I understand completely.

WALL: It also demonstrates that there are human frailties. It gets students out of the idea that if something is in print it must be sacrosanct. Don't you think that's important?

BOHNING: Yes, absolutely.

WALL: I generally would lecture without recourse to notes. I might make notes of some precise numerical information I needed, but for derivations, I would just start in on the black-board, write and erase and call on students. I'd often get to a certain stage and then I'd say, "How about it? What do you think we ought to do next?" I wanted the students to participate. That was fun, and I think the students enjoyed it.

In this connection, I did have one embarrassing situation that happened near the end of the class hour. I had about a minute to go, and I wanted to complete something, but I was on the wrong track. I was obliged to admit that, "I'm sorry I don't know just exactly what I'm supposed to do now, but I'll tell you next class hour." I couldn't make a new start because the bell rang.

Years later, one of my students remembered that incident. There was a chemical engineer by the name of Tom Baron who became president of Shell Research, which had moved

down to Houston. I did some consulting, incidentally, for Shell when I was at Rice in Houston. I did it because there was no travel involved. Anyhow, I had lunch with Tom Baron, and he said he remembered my lectures and how he enjoyed them. Then he remembered this incident I cited. He didn't identify it, but I knew what he was saying. He said, "You know, the thing I liked was that you would do a problem without notes, and if you got into a mess, you could even laugh at yourself." That was a compliment, I thought. I did get into a mess, and that was not the only mess, but usually I got out of them more gracefully.

The consulting at Shell was relatively brief, but it was interesting. Shell had moved from Emeryville, in the San Francisco Bay area, to Houston, and that was a traumatic company experience. They had lots of resignations; divorces and unhappiness in the families of people who didn't want to leave the nice Bay area to go down to that God-forsaken, hot Houston. Shell moved down there and the initial morale was rotten. It was so rotten, in fact, that I found that Tom wanted me to be a consultant. He knew I'd consulted for DuPont in past years, and he thought maybe I could help out. It turned out that in a way that resembled my experience after leaving Illinois. However well I might have done at DuPont, the people at Shell didn't trust me. Chemists I talked to wouldn't tell me what was going on. For all I know, they might have thought that I was a planted spy. I remember in one instance, Tom asked me to look into a certain problem. I did some background work and then talked to the person who was working on it; he didn't tell me anything that he knew about the subject.

Shell has a new generation now, resulting from attrition, retirements, new hiring, and normal expansion. By now, they've got new people. It was a hell of a mess. So, I looked back with nostalgia at DuPont. [laughter] You can understand why.

BOHNING: Yes. And Illinois.

WALL: Yes, and in both there are somewhat similar ways.

BOHNING: Yes. That's interesting.

WALL: The only trouble with consulting for DuPont was that it tended to pull me away from pure inquiry and my family. These were both my fault, not DuPont's.

BOHNING: I'd like to come back if I could to a little bit of the Illinois period.

WALL: Sure.

BOHNING: You wrote a paper with Paul Flory (18).

WALL: Yes.

BOHNING: I was intrigued by how that association came about.

WALL: Oh, Paul and I were pretty good friends. Paul had done some work that I studied, and on which I worked further. So we got to know each other and chat with each other. We met occasionally, and then I developed my theory of rubber-like elasticity that differed from a theory of Eugene Guth and James. Guth was inclined to be polemical and wrote about disagreeing with me.

BOHNING: Ah, yes.

WALL: Paul took my side, but I was willing to forget it. However, Paul was pretty strong-minded and inclined to be quite vocal about his positive feelings and he had no compunction about writing about such things. He wrote a lot more than I did. Evidently, he enjoyed writing, whereas I found writing difficult. I hate to admit this, but I'd have written more if I had any real fun writing. I had more fun finding things out, and I have much unwritten material that would have been grist for the mill at an earlier time. But then I look back and say, "It's just as well I didn't write it up. What difference did it make?" Early on it would have helped me get tenure, but then, I got tenure fast enough, so what's the difference? Anyway, Paul said, "Fred, this guy Guth is really doing you dirt. He shouldn't do that. You don't like that, do you?" I said, "No, I don't like it." "Well, why don't you write up something and show him where he's wrong?" I said, "No. I'm not going to write up something. Why should I bother to write up something to show him he's wrong?" Then Paul said, "Well, why don't we write it together then?" So, we wrote it together, although I think he wrote most of it. [laughter]

BOHNING: I looked at that paper. I think I have a copy.

WALL: Well, he threw in a phrase that I don't think I would have put in, something like this settles it, or words to that effect.

BOHNING: I guess the first reference is to Guth. He said, "We shall accordingly restate some of the principles which form the basis of the theory so vigorously criticized by Guth, and we will also point out the untenable implications of their ultimate hypotheses."

WALL: Yes, that's Flory's language. I shared that and I accepted it.

BOHNING: Oh, here we are. "We believe the forgoing discussion disposes of the major points of disagreement."

WALL: Yes, that was Paul's statement. [laughter] You may keep this if you want. I don't need it. I've got reprints.

That's the history of that paper, and I don't think Paul and I added anything to the science. It was—I hate to admit it—really polemical.

BOHNING: That's interesting.

WALL: Yes, that's what it was. You read that last sentence and you read the early part: you can see it was polemical. Actually, Paul egged me on.

BOHNING: The other thing that I wanted to talk about was your experiences in the Rubber Research Program.

WALL: Yes.

BOHNING: You published a lot of things after the war was over (19), although you had been involved in some of the theoretical work before that. I'm just wondering if you can share some of your experiences with the Rubber Research Program during World War II.

WALL: I mentioned earlier how this program started. Speed Marvel was in charge of the program at Illinois, and I was in charge of the physical chemistry part thereof. Herb [Herbert] Laitinen was in charge of the analytical chemistry part. What we did was to study polymerization kinetics and molecular weight distributions of the rubber polymers. We also studied certain catalytic systems, one involving an oxidation-reduction cycle that promoted a chain reaction. We looked into those things. It was part of a rather substantial and vigorous program.

BOHNING: Were there any particular people you interacted with in that program outside of Illinois?

WALL: Yes. One person really stands out, namely [Peter J.] Debye. He was at Cornell, and he participated in the program. Debye was a remarkable individual. In fact, he was kind of an ideal for me. I often wished that I could be like Debye: level headed, exceedingly pleasant, a good sense of humor, a nice guy, but not a nonsense individual—just an out-and-out collections of brains with a capacity for good judgement. He was involved in this program and used his experience as an academically-oriented scientist to help solve some practical problems in connection with the rubber program. He developed a light scattering business for characterizing polymer solutions. He was really a remarkable individual, and I communicated with him about macromolecular configurations, especially after the war was over, when it became more academic. The program continued for a while even after the war. When we talked about science, he would sometimes say, “You don’t really mean that, do you? Shouldn’t you think about this?” He had a way of eliciting from your own thoughts the answer to a problem that was perplexing you.

I remember once when I gave a talk at a meeting in Chicago. This was an ACS meeting, I believe, and he was at the meeting. I don’t even remember precisely what the talk was now, but I remember this occasion because he asked about something related, and I said, “I don’t know.” He said, “Well, you can figure that out.” I said, “I’m not certain how.” This was with an audience present, and he said, “What you do is, you do this and then you do this and do this. You can do it.” [laughter] This was in front of everyone, and I wasn’t all that sure of myself. The next time we saw each other, he asked, “What did you find out?” I told him I had worked on it, and I told him what I’d found out. It turned out not to be particularly involved or enlightening. Maybe he was disappointed in what I told him, but anyhow I had tried what he had said.

I remember another incident where Debye really endeared himself to me. I was at a Gordon Research Conference, and I gave a talk about the behavior of polymeric electrolytes. I got interested in polymer electrolytes, to learn about polymer motion in electrical fields. We showed that if you electrolyze a solution of polyacrylic acid partially neutralized with sodium hydroxide, more sodium can move to the anode than to the cathode. The reason is that the large tremendous negative ion sequesters a lot of the sodium and carries it along in the direction opposite to where a positive ion would normally go. We unequivocally established such sequestering. That was something that John [R.] Huizenga did with me (21). He had worked at Oak Ridge.

BOHNING: Yes.

WALL: At any rate, I was interested in polymeric electrolytes, and I was giving a talk on polymeric electrolytes at the Gordon Research Conference. Two of the members in the audience were Debye and [Lars] Onsager. I can tell you an anecdote about Onsager. Anyhow, Onsager and Debye were there, and Onsager asked me a question. He said, “You wrote down such and

such; that's not the way polymers behave. The equation ought to be this." I replied, "No, I don't think so. It ought to be what I showed." Onsager was certainly a highly regarded and distinguished person, and one doesn't want to quarrel with him at a meeting. He was pretty adamant. This exchange went on a little bit. Then Debye spoke up; he pointed his finger at Onsager and said, "See here, Lars. Wall is talking about coiling-type molecules, not your sticks." And Onsager shut up. [laughter] Onsager had, indeed, studied viscosity theory in which he had envisioned stick-like molecules, and I was dealing with a coiling type. Debye knew that Onsager was talking about one thing, and I was talking about another. It hadn't occurred to me to challenge Onsager directly. [laughter] So you can understand why I have a soft spot for Debye.

BOHNING: Yes.

[END OF TAPE, SIDE 7]

WALL: Debye with [Erich] Hückel worked out a theory of ordinary electrolytes, and this had an error in connection with electrical conductivity. Evidently Onsager recognized this deficiency. One day a young tall blond shows up at Debye's office, in Zurich, I believe, but anyway, somewhere in Switzerland. Debye was a professor at one of the universities there. This blond Norwegian comes and looks in the door and said, "Are you Professor Debye?" "Yes." "Your theory is wrong." So Debye said, "Come in, be seated, and tell me what's wrong." Then he started talking about the electrical conductivity problem. "You know," Debye said, "Onsager was right." [laughter] Debye told this story himself. So, I think he had a perfect right to say, "See here, Lars." [laughter] I recall that because he was not being arrogant. He was just treating another person on equal terms, so to speak. But he could be very gentle.

When he wrote an article once for *Scientific American*, he asked me for some information and thoughts that he might convey about macromolecular dimensions. So I sent him some material. Several months later, I got a letter from him. (I had forgotten about it and I hadn't seen the article.) He said, "The article appeared in *Scientific American*, but I have to apologize for it. They rewrote it and they left out what I thought was important. They left out what I considered best; they left out some of the stuff you gave me, and I hope you'll understand the reason for it." He took the trouble of telling me that.

I also remember that Debye was one of the first people on the Welch Foundation Scientific Board when it just started. They were looking for a scientific director and Debye asked me if I might be interested in it. I thought it over, but regretfully declined. I said I wanted to stay at Illinois, closer to direct academic work and research, rather than get involved in the foundation job. Although I never got a firm offer, I imagine that his recommendation would have carried the day, because he was at that time calling the shots.

All I know is that when I made decisions to go away like the ACS and so on, that was a terrible mistake; maybe I should have weathered the storm at Illinois. Well, I don't mean to say that my life was unhappy. Oh, quite the contrary, that's why we moved back here. I mean, we do like the environment; we have friends at the university, although they're diminishing. One of our best friends—Oh, Harold [C.] Urey, I can tell you anecdotes about Urey.

Speaking of foundations, I was offered another foundation job, at the Sloan Foundation. They wanted me to head up the scientific part of it. That was when I was in my heyday as dean, at a time when I wasn't thinking about anything else, didn't want anything else. It was when I wanted something else that these things didn't come. Rather than go to Santa Barbara, I might well have gone to Sloan, maybe even to Welch, who knows. I don't know, but there was a period when everything was running for me, and then things seemed to falter.

Now, I think I interjected a comment earlier about political methods getting me into the National Academy of Sciences. I did not, myself, conduct such a campaign, except unwittingly. I use the word unwittingly because here's what happened. Life was going well and the research was going well at Illinois, and I had heard that I had been nominated, and that it's hard to get in. Of course, I wanted it and loved the idea. But I wasn't getting around and talking to people. I wasn't giving lectures. I did the work but I was so preoccupied with deaning and consulting that I stopped going to meetings to give papers.

Then Herb [Herbert S.] Gutowsky came to me one day and said, "Fred, you know, you've done some interesting work. You really ought to talk about it." I said, "Where? Nobody asks me to talk anywhere." "Well, I'll tell you, I think maybe they'll ask you." So, Herb Gutowsky—I believe it was he—arranged for me to give lectures at Princeton and Harvard and maybe another school in the east. I talked about the computer calculations of reaction probabilities. I remember going to Princeton, where Hugh Taylor was still dean of the graduate school. (He was a physical chemist, too.) I remember his coming to the lecture, and he came to me beforehand and he said, "I hope you'll forgive me if I have to leave a few minutes early, but I have such-and-such a commitment. But, I certainly wanted to come and hear you." He was very generous, you know, colleague to colleague, one dean to another. I gave the lecture and it seemed to go very well.

Then I went to Harvard and that lecture went very well, too, and I was delighted. I remember they played a little trick on me at Harvard. When I was finished, I said, "I believe that is the last slide and I'm through." Whereupon the lantern slide operator said, "Oh, no, there's one more." I said, "One more? I didn't think there were anymore." He said, "Well, it's a picture of the machine you used for the calculations." He put on a slide and there was a picture of a cement mixer. [laughter] Everybody laughed and we had a lot of fun.

But that wasn't all. Once again, I believe Herb arranged for me to give lectures at Berkeley, UCLA, and Caltech, on the same subject. Herb never said so, but I knew very well what he had in mind. Those are the places where academicians are. "You must go talk to those guys." Within two years, I was elected. I'm not sorry it happened, but you begin to ask what goes on in this world.

BOHNING: Yes. I understand exactly what you're saying.

WALL: Oh, I can tell you another interesting anecdote. Do you mind?

BOHNING: No, I don't mind.

WALL: I was going to tell you an anecdote about Willard Libby, who was at UCLA. I believe it was the night I gave that lecture. I'm sure it was the same lecture; maybe it wasn't on that same trip, but that's immaterial. My wife was with me at the time, and we went for dinner to the house of one of the members of the department, Bob Scott's house. Bill Libby was at the dinner and there were several other people there, too. At that time, Bill was much preoccupied with making air raid shelters. He had started a series of newspaper articles on how to build your own shelter, and to protect yourself against radiation in the event of nuclear attack.

He said, "Of course, if you can't build a shelter as you really ought to, then I can tell you what to do in your own house. Let's go down to the basement here and I'll show you where you should go if there were an attack." So, we went down to the basement, and he said, "Now, if there were an attack at"—and he named the Air Force base in north Santa Barbara somewhere—"you should get into this corner of the basement and wait. But it is better to make a shelter. I'm writing a series of articles on it," he said. "You can make it and equip it for your labor plus about twenty-five to thirty dollars worth of equipment that you would have to buy. In other words, it's not going to be a big investment except your sweat." I asked, "How do you do it?" "Well, the best place, of course, is to have a hillside. If you have a hillside, you can dig a hole into the hill, put some steel beams across, and then pile some dirt on top. You can have a little transistor radio and some supplies in there, and if you're on the right side of the hill, everything's fine."

I said, "That sounds pretty good. But, tell me, Bill, if you're going to put steel beams across there, how are you going to do this for a mere twenty-five to thirty dollars?" He said, "Well, I go over to the junk yard at the university physical plant and I can find these things there." I said, "Yes, but Bill, if you're writing this series of articles for people all over the country, they can't go to the university plant junk yard and pick up steel beams. Not if this is going to be a practical thing. Now, really, how are your readers going to do it?" So, he sort of stroked his chin, "You've got a point there, Fred. You know, I guess I should take into account how people get these things." So, he changed his plans. He said, "You can use wooden beams, like railroad ties." You can get railroad ties for landscape purposes and there was a time when you could get them at little cost. I do not know what they cost.

Anyway, he built his air raid shelter and used railroad ties to hold up a pile of dirt. I think his series of articles was just starting, when there was a severe brush fire. That brush fire

burned up that hillside and burned the railroad ties so that the roof caved in. I never had the heart to speak to Bill Libby again about that, but he stopped his newspaper series. He took a lot of ribbing, but he never said a word to me. For all I know, maybe he forgot.

Though Libby and I were pretty good friends, we didn't see a lot of each other, to be sure. Bill was very conservative in his attitude. He supported Ronald Reagan, our governor at the time. I was not similarly inclined. Bill also wanted to be president of the University of California. One time when my wife and I were in New York, we had dinner with Gordon Ray, head of the Guggenheim Foundation. Bill Libby was a guest, too. Bill was on his board for recommending fellows and so on. That night, Bill had too much to drink. We got to talking about the university and all of a sudden he turned to me and said, "Fred, where were you when they were picking a new president of the University of California? Why weren't you speaking up for me?" That came out of the blue. Gordon Ray apologized the next day to us for what Bill said. He thought it was disgraceful. I hate to repeat this event, but I'm just telling you what I recall.

There's one interesting off-shoot which relates to my experience at the university here in La Jolla. It was the last year I was here. The university was thinking about the business of how to deal with retirees, people who had reached what was then considered the normal retirement age. There was talk about extending it. It was felt that it would be a good idea to handle matters on an individual basis and to extend the appointment if it was felt that the individual could make a useful contribution. The trouble with that was if it is made possible to extend an appointment, people begin to assert that as a right. You invariably run into that kind of problem. Whenever there's a judgment factor, who wants to say, "But in our judgment you're not the person who ought to have his appointment extended."

Along about that time, there was a political activist at UCSD by the name of Herbert Marcuse. Herbert Marcuse was the darling of the left, but he had reached retirement age, so the question of his continued appointment came up. Students seemed to love him, especially those who were inclined to go in for demonstrations. He was their hero. When students protested, Marcuse would champion their cause. Earlier, I had talked with him about the nature of his supporters and he said, "You know, they're not very good," or words to that effect. It's funny, he was their champion, but he didn't think well of them.

When the question of Marcuse's reappointment came up, the question of Pauling's reappointment at UCSD also arose, and various people, including me, were asked for comments. I was familiar with Pauling's credentials, and I looked at what Marcuse had been doing. I made note that Marcuse hadn't been doing any scholarly work for a number of years, and under such circumstances I really didn't think an exception should be made for him. In the case of Pauling, who was still working, I saw a different situation. So, I split on the two and recommended in favor of Pauling, but not for Marcuse. Interestingly enough, the fact that I had said that Marcuse probably ought not to be reappointed got to then-governor Reagan's attention, and I learned later that he was pleased. I didn't know that at the time, but subsequently, Bill Libby and I were discussing the matter, and Bill Libby told me about a possible future for me. He said, "You know, Fred, the governor thinks well of you." Why should he think well of me? The only thing

that occurred to me was this business about my not recommending Marcuse for continued employment.

My recommendations had nothing to do with politics. Absolutely nothing. It's just that Marcuse hadn't been doing anything except getting students to demonstrate, and I thought that was hardly a reason for keeping him on campus after retirement age. I never voted for Reagan and I didn't share his political thoughts. Anyway, Bill Libby told me about how I was in good graces with the governor.

Now, let's see, there's Harold Urey.

BOHNING: Yes, I was going to ask you about him.

WALL: We were quite close to the Ureys. Clara and Freda Urey were very close friends. We would share meals and do other things together. This persisted even during the years after Harold had difficulty moving around. The one place he would come to was to our house. He died then some years ago, but Freda is still alive; she's ninety-three or ninety-four, something like that, but much more frail than she used to be. A remarkable woman. Clara sees her periodically, and we keep in touch.

Harold Urey was a pretty tough scientist in his early days. He softened up more in later years. He was very understanding about things in general, and he was a defender of the right to express thoughts whether he shared them or not. He really was a libertarian in the sense that he felt that you've got a right to speak.

I remember once when Urey came to the University of Illinois, before I was a dean. He gave a lecture on the moon. (He got into geology and the moon business almost immediately after the war.) He was introduced by Worth Rodebush, who liked to play little jokes on people. Rodebush said, "Urey, of course, has distinguished himself in so many ways; although he discovered deuterium, he's never had an element named after him, however, and probably won't until he discovers a halogen." Well, Urey remembered that because years later when we were here, I said to Harold, "I remember when you gave a lecture at Illinois." He said, "That lecture. Oh, I remember. That's when Rodebush said something about my needing to discover a halogen." So, that stuck in his mind, and I had forgotten about it until he reminded me of it.

BOHNING: Where did you first meet him? Here or at the university?

WALL: I think the first time I met Harold Urey was at an ACS meeting where we happened to be at the same table eating, but that was all. Actually, when I was nearing the end of my graduate work, I applied for a post-doctoral position with Harold Urey when he was at Columbia. At the same time I applied at Illinois and so on. Urey expressed interest in some of

the thoughts I had. I had written up some things that I was thinking about. I was neither accepted nor rejected by him because the Illinois position came up. I was glad, of course, to go to Illinois and have what presumably was to be a steady position. I really met Harold for the first time when he came to Illinois to give that lecture. Of course we got to know him well in La Jolla. Interestingly enough, our connections here were principally social. They weren't scientific, but we, and our wives, in particular, saw a lot of each other on a purely social basis.

[END OF TAPE, SIDE 8]

BOHNING: Well, I've reached the end of my questions, do you have anything else you want to add?

WALL: Oh, I don't know. I could keep talking indefinitely, and I shouldn't.

BOHNING: If you have more, I'll be glad to listen.

WALL: Well, I don't know. I don't want to be self-serving about things, so I'm not going to talk about what was or was not valuable about what I did. All I can say is that I reached my peak in terms of overall productivity at Illinois while I was an administrator. That's an odd combination, but that is true.

BOHNING: Yes, it is.

WALL: That's when I got some recognition, both administrative and scientific, job offers and things like that; subsequently, there was a tapering down, reflecting the practical realities of the way this world functions.

BOHNING: What about the ACS? Unless you don't want to talk about that?

WALL: Oh, I'll tell you about it.

BOHNING: You passed over that rather quickly, and you were there for three years.

WALL: Well actually, it was a mess. I knew it was a mess, but I thought that I could help straighten it out. For one thing, they didn't really want an executive director. They hadn't had an executive director before, and the board said they wanted somebody who could manage it. Well, it turned out that the existing staff didn't want an executive director either, even though the board had said to the contrary.

When I showed up; they didn't even have an office set aside for me. It is not pleasant to boot somebody out of an office. Surely, the chairman of the board could have said, "Here's where your office will be. These other people are going to go somewhere else." I was supposed to be the top dog but I didn't have an office. There were little things, often trivial and bordering on perquisites which didn't concern me, except for the symbolism involved. I traveled by coach class accommodations; my immediate subordinates traveled first class. My actions were resented, but I won't go into detail.

I was supposed to act like a big shot, never mind if I did anything useful. This got more and more involved, not to mention the fact that there were countless committees made up of members who do various things. I don't know what the situation is today, but at that time, they had a committee on membership, which was unduly sticky about enforcement of rules. I learned that there was a physicist working at Bell Labs who had gotten involved in polymer work, and had done a lot of what I would call physical chemistry. His undergraduate record didn't have as many hours of chemistry as expected of chemists, but he had a lot of chemistry, and he had a lot of physics and mathematics, and then he had experience, publications and so on. I said, "A fellow like that wouldn't hurt us." You see, the committee had turned him down, and his case was appealed to me. I said, "This is crazy. This man is perfectly all right." Later, we started rotating committee membership. I wanted such rotation, and happily we had a good president who shared my views.

BOHNING: In the early 1970s?

WALL: Melvin Calvin was president, and he and I saw eye to eye. There was no question about it. We started revamping things, but it turned out that members who had been displaced from committees started raising a stink through various channels, saying, "I've been doing this for twenty years. Who has more experience than I?" Then the board begins to hear that Fred Wall is making things rough on people who had been loyal members serving the society all these many years. Well, this sort of thing wears one down, leaving little chance really to do something in a meaningful way. Incidentally, Melvin Calvin was a colleague of mine at the University of Minnesota. He got his Ph.D. under George Glockler.

BOHNING: Really?

WALL: Yes.

BOHNING: I didn't know that.

WALL: Oh, sure, he got it under George Glockler. He finished up one year ahead of me. When he got through, there still weren't any jobs available. But he wrote around, all over, including to Polanyi at Manchester. He got an appointment there. I've forgotten what the benefits were. He got perhaps a hundred pounds or something like that, back in those days worth four or five hundred dollars, no magnificent sum. But it was something that, with perhaps a little bit of help, he could manage, and that experience did him well, I'm sure. At any rate, he was then able to get what was essentially a post-doctoral position with G. N. Lewis at Berkeley, and that's how he got started at UC. Then he got a teaching position, and started moving up fast. Melvin Calvin is, I would say, the most distinguished of Glockler's students.

BOHNING: So, in terms of the ACS, then, was that it?

WALL: It was an unhappy experience for me, mainly because my desire to make it a meaningful place just didn't click with some of the old guard on the staff and on the board. The board is made up of people who, for the most part, were chemists who did have industrial connections. Academia was not all that well represented on the board. There were some. In the case of the president of the society, there was an unwritten rule that they would alternate between academic and industrial. The academics could generally elect a president if they wanted and the reason is interesting. This was true not because there are more academics among members. Two-thirds or three-fourths of the members are industrial chemists, but the bench chemist would rather vote for a professor than for a manager. That is actually what happened.

In the old days, ACS had a policy in which the members by vote could nominate two candidates, the two highest would be the nominees. Then the board or the council would elect the president from those two nominees. When Pauling's name was first put up as president of the ACS, he ran a landslide against the second candidate, but the second candidate (who was from industry) was elected by the council, maybe in deference to the unwritten rule that it was an industrial year. The next year they elected Pauling.

At the same time there was a move afoot promulgated by a fellow who had been at Shell, Emeryville, who wanted the ACS to become more like a bargaining agent for chemists. Their group said, "We need to stick up for chemists' rights," and so on, and they were pushing hard for trying to convert the ACS. I talked with them and I said, "I have no quarrel with your idea that chemists ought to be represented, but why do you say the ACS should represent them? The ACS is organized under its charter to support the cause of the science of chemistry, and as such it can draw members from industrial management, academics, and from research chemists—without reference to whether you're an employer or employee, without reference to whether you're industrial or academic. If you're a chemist, that is the first thing that counts in expecting

the society to do something for you. That being the case, the society cannot undergo a cleavage by saying, 'We are now going to champion the cause of one group, however large it is, at the expense of another part of our society.'"

I said this on what I thought was a rational basis. I had no objection to their having a union; if they wanted it at Emeryville, it's okay with me, but don't turn the ACS into a union. Now, by the same token, I was not inclined to suggest that the industrial bias reflected by some industrial domination of the ACS was a good thing. It was not. I was opposed to that, too. I didn't like that bias. I wanted more of a push for chemistry. I think you can infer that from what I've said.

BOHNING: Sure.

WALL: That was a hopeless task. There had been some reforms; one of them was a reform in *C&EN* [*Chemical and Engineering News*]. The time I went there it was purely and simply a weekly sheet or magazine for industrial news. The idea that they'd ever report any interesting scientific development was just taboo. The editors said, "What we do gets us advertising and advertising makes money; if we make money, see, what more do you want? We're not a drain on the society; we're a money source." To make money is all right; if you don't make money, it isn't all right. So I said, "*C&EN* is a magazine to provide news for members, and it's also the official organ of the society. It should also give information about what goes on in the society and the essence of what is going on in science."

The initial response was, "What's in it for us if we do that sort of thing?" But they have actually changed some. You'll see some scientific articles in *C&EN*, some summaries, and I have been heartened to read recently about new discoveries. Twenty years ago, they wouldn't have touched it.

So, I was happy to get out. I think the board was happy to have me go, to be perfectly honest. So I went back to academic work, got back into research and though it wasn't my hey-day, it was respectable at Rice.

BOHNING: How did you pick up the Rice position?

WALL: Well, I was fortunate there. I've been lucky in my day. I had occasion to chat with Nate Newmark of Illinois and I told him that I was going to leave Washington. A short time later, I got a call from him asking if I'd come back to the University of Illinois to head up their Institute for Advanced Study, something that I had been instrumental in setting up under the auspices of the graduate college. This was for people who didn't have administrative positions, and it provided internal recognition within the university.

I had also been approached about being president of a college, but my wife talked me out of that, and she was right. I made no effort to pursue it. I did, however, get an offer from Rice, which I accepted. Then we thought about building our house in La Jolla, and I taught for awhile at San Diego State University. I learned something there. I learned that the best people at SDSU were better than the less able people at UCSD. In other words, there's an overlap. Sometimes it's luck where we end up.

Think about the baseball teams. Even the weakest team in a major league will have at least one player that the top team would want. Witness all the trading that goes on. It's one thing to have the best team; it's another thing to say that everyone on the top team is better than anybody else on a bottom team, which is an idea that some people have in the university. Having seen it from both sides, I know that that is just plain foolishness.

I think about that and I say, "That's unjust." I don't know the solution, but I know it's unjust. I can say it because I've been in both. I've been in the University of Illinois, a big university, and I've been at two branches of the University of California. I have also been at a first-rate private school—namely, Rice. And I've been at a second-tier type of institution and I sympathize with those poor devils who work like dogs, with excessive teaching loads and struggling to try to get a little research recognition. They're lucky if they can publish a paper or two out. I can express a sense of social responsibility within academia—that's a feeling of mine that I have seen and observed and to which I can testify.

BOHNING: Well, on that note, is there anything else you'd like to add? If not, why don't we stop at this point, and let me thank you again very much for spending the time.

WALL: Well, I hope I didn't ramble on too much.

BOHNING: No, not at all. I've enjoyed it very much.

[END OF TAPE, SIDE 9]

[END OF INTERVIEW]

NOTES

1. Frederick T. Wall and George Glockler, "The Raman Effect of Deuteroammonia," *Journal of Physical Chemistry*, 41 (1937): 143.

Frederick T. Wall and George Glockler, "Raman Effect of Gaseous Methyl- and Dimethylacetylenes," *Physics Review*, 51 (1937): 529.

Frederick T. Wall and George Glockler, "The Double Minimum Problem Applied to the Ammonia Molecules," *Journal of Chemical Physics*, 5 (1937): 314.

Frederick T. Wall and George Glockler, "Bond Force Constants and Vibrational Frequencies of some Hydrocarbons," *Journal of Chemical Physics*, 5 (1937): 813.
2. Malcolm Renfrew, interview by James J. Bohning at New Orleans, Louisiana, 31 August, 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0076).
3. Frederick T. Wall and G. W. McMillan, "Infrared Absorption of the Phenylmethanes," *Journal of the American Chemical Society*, 61 (1939): 1053.

Frederick T. Wall and W. F. Claussen, "Infrared Absorption Studies of Some Glycols Ethoxyalcohols," *Journal of the American Chemical Society*, 61 (1939): 2679.

F. T. Wall, A. M. Buswell, G. W. McMillan, and W. H. Rodebush, "Infrared Absorption Studies. VIII. Hydrazoic Acid," *Journal of the American Chemical Society*, 61 (1939): 2809.

Frederick T. Wall and W. F. Claussen, "Infrared Absorption Spectra of Some Carboxylic Acids and of Dibenzoylmethane and Related Molecules," *Journal of the American Chemical Society*, 61 (1939): 2812.

Frederick T. Wall and G. W. McMillan, "Infrared Absorption Studies of Some Hydrocarbons," *Journal of the American Chemical Society*, 62 (1940): 2225.
4. Frederick T. Wall, "Ionic Character in Diatomic Molecules," *Journal of the American Chemical Society*, 61 (1939): 1051.
5. P. J. Flory, "Principles of Polymer Chemistry," *Journal of the American Chemical Society*, 61 (1939): 1518.
6. F. T. Wall, "Removal of Substituents from Vinyl Polymers," *Journal of the American Chemical Society*, 62 (1940): 803.

F. T. Wall, "Removal of Substituents from Vinyl Polymers. II," *Journal of the American Chemical Society*, 63 (1941): 821.

F. T. Wall, "The Structure of Vinyl Copolymers," *Journal of the American Chemical Society*, 63 (1941): 1862.

- F. T. Wall, "Intramolecular Condensations in Polymers," *Journal of the American Chemical Society*, 64 (1942): 269.
7. F. T. Wall, L. A. Hiller, Jr., and D. J. Wheeler, "Statistical Computation of Mean Dimensions of Macro-molecules. I," *Journal of Chemical Physics*, 22 (1954): 1036.
 8. Frederick T. Wall, Walter E. Mochel, and William H. Sharkey, U.S. Patent No. 3,076,789: Polyamides from Diamines Having Two Deuterium Atoms on Both Chain Carbons Alpha to the Two Amino Nitrogens. Assigned to E. I. duPont de Nemours and Co., Inc., February 5, 1963.
 9. Frederick T. Wall and George Noel Milford, Jr., U.S. Patent No. 2,976,268: Polymer of 2, 6-Disubstituted Heptadiene-1, 6. Assigned to E. I. duPont de Nemours and Co., Inc., March 21, 1961.
- F. T. Wall, A. G. Armour, and M. Fryd, Belgian Patent No. 667,015: Dispersion de Polymeres Synthetiques dans de Liquides Organiques. Assigned to E. I. du Pont de Nemours and Co., Inc., January 17, 1966. Patents in other countries pending.
10. Rachel Carson, *Silent Spring* (Greenwich: Fawcett Publications, 1962).
 11. F. T. Wall, L. A. Hiller, Jr., and J. Mazur, "Statistical Computation of Reaction Probabilities," *Journal of Chemical Physics*, 29 (1958): 255.
 12. John Polanyi, Nobel Prize Address, reprinted in condensed form in *Science*, 236 (1987): 680.
 13. Frederick T. Wall and F. Mandel, "Theory of Rubberlike Elasticity," *Journal of Chemical Physics*, 64 (1976): 1998.
 14. Frederick T. Wall and F. Mandel, "Macromolecular Dimensions Obtained by an Efficient Monte Carlo Method Without Sample Attrition," *Journal of Chemical Physics*, 63 (1975): 4592.
 15. Frederick T. Wall, "Discrete Wave Mechanics: An Introduction," *Proceedings of the National Academy of Science*, 83 (1986): 5360.
- Frederick T. Wall, "Discrete Wave Mechanics: The Hydrogen Atom," *Proceedings of the National Academy of Science*, 83 (1986): 5753.
- Frederick T. Wall, "Discrete Wave Mechanics: The Hydrogen Atom with Angular Momentum," *Proceedings of the National Academy of Science*, 84 (1987): 1469.
- Frederick T. Wall, "Discrete Wave Mechanics: Multidimensional Systems," *Proceedings of the National Academy of Science*, 84 (1987): 3091.
- Frederick T. Wall, "Discrete Mechanics and Special Relativistic Random Walks," *Proceedings of the National Academy of Science*, 85 (1988): 2884.

16. Frederick T. Wall, *Chemical Thermodynamics* (San Francisco, W. H. Freeman and Co., 1958).
- Frederick T. Wall, *Chemical Thermodynamics*, Second Edition, revised (San Francisco, W. H. Freeman and Co., 1965). Reprinted as International Edition by Toppan Co., Ltd., Tokyo, Japan.
- Frederick T. Wall, *Chemical Thermodynamics*, Third Edition, revised (San Francisco, W. H. Freeman and Co., 1974).
17. Richard C. Tolman, *Relativity, Thermodynamics and Cosmology* (Oxford, 1934).
18. Frederick T. Wall and Paul J. Flory, "Statistical Thermodynamics of Rubber Elasticity," *Journal of Chemical Physics*, 19 (1951): 1435.
19. F. T. Wall, "Polymer Properties as Functions of Conversion," *Journal of the American Chemical Society*, 67 (1945): 1929.
- F. T. Wall, F. W. Banes, and G. D. Sands, "The Mechanism of Modifier Action in GR-S Polymerization. II," *Journal of the American Chemical Society*, 68 (1946): 1429.
- F. T. Wall, R. W. Powers, G. D. Sands, and G. S. Stent, "Properties of Polymers as Functions of Conversion. II. Intrinsic Viscosities," *Journal of the American Chemical Society*, 69 (1947): 904.
- F. T. Wall and L. F. Beste, "Properties of Polymers as Functions of Conversion. III. Molecular Weights of Bottle Polymerized GR-S," *Journal of the American Chemical Society*, 69 (1947): 1761.
- F. T. Wall, R. W. Powers, G. D. Sands, and G. S. Stent, "Properties of Polymers as Functions of Conversion. IV. Composition Studies of Rubber-Like Copolymers," *Journal of the American Chemical Society*, 70 (1948): 1031.
- F. T. Wall and T. J. Swoboda, "An Oxidation Reduction Cycle in Emulsion Polymerization Systems," *Journal of the American Chemical Society*, 71 (1949): 919.
20. F. T. Wall, J. R. Huizenga, and P. F. Grieger, "Electrolytic Properties of Aqueous Solutions of Polyacrylic Acid and Sodium Hydroxide. I. Transference Experiments Using Radioactive Sodium," *Journal of the American Chemical Society*, 72 (1950): 2636.

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